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DYNAMICS OF PERSONALITY ORGANIZATION

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V. THE ORGANIZATION OF THE PERSONALITY SYNDROME

In Part I of this paper we have spoken as if the various parts of the syndrome are homogeneous, like the particles in a fog. Actually this is not the case. Within the syndrome organization we find hierarchies of importance, and clusterings. This fact has already been demonstrated in the simplest possible way for the self-esteem syndrome, namely by the method of correlation. If the syndrome were undifferentiated, any part of it should correlate as closely with the whole as any other part. Actually, however, self-esteem (measured as a whole) correlates differently with various parts. For instance, the whole self-esteem syndrome as measured by the Social Personality Inventory correlates with irritability $r = -.39$, with the pagan sexual attitude $r = .85$, with a number of conscious inferiority feelings $r = .40$, with embarrassability in various situations $r = -.60$, with a number of conscious fears $r = -.10$.

Clinical examination of the data also shows this tendency toward a natural clustering of the parts into groups which seem intrinsically to belong close together. For instance, conventionality, morality, modesty, and regard for rules seem to fall together or belong together very naturally, as contrasted with another group of clustering qualities, such as

self-confidence, poise, unembarrassability, lack of timidity and shyness.

This tendency to cluster at once gives us the possibility of classifying within the syndrome, but when we actually attempt to do this we are presented with various difficulties. First of all we are confronted with the common problem of all classifications, that of the principle upon which the classifying shall be based. Of course if we knew all the data and all their interrelationships, this would be easy. But when, as in our case, we proceed in partial ignorance, we find that we must be arbitrary at times no matter how sensitive we try to be to the inner nature of the material. This inner hanging-togetherness gives us, in our case, an initial clue, an indication of general direction. But we can go only so far with such spontaneous groupings, and when we finally come to the point that we fail to perceive them, we must proceed on the basis of our own hypotheses.

Another apparent difficulty is that when we work with syndrome materials we soon find that we can classify any personality syndrome into a dozen main groupings, or a hundred, or a thousand or ten thousand just as we please, depending upon the degree of generality that we have in mind. We suspect that the usual attempt at classification is simply another reflection of the atomistic, connectionistic point of view. Certainly the use of an atomistic tool in dealing with interdepending data cannot get us very far. What is classification ordinarily if not the separation out of different parts, of discrete items? And how then shall we classify if our data are *not* essentially different and separate from each other? Perhaps we shall have to reject atomistic classification and look for some holistic principle of classifying just as we found it necessary to reject reductive analysis in favor of holistic analysis. The following analogies are offered as indications of the direction in which we must look for such a holistic technique of classification.

Levels of magnification.—This phrase is a physical analogy derived from the way in which a microscope works. In studying a histological slide, one gets the whole-character,

the general structure, the formation and interrelationships in their totality by holding the slide up to the light and looking at it with the naked eye, thereby encompassing the whole. With this whole picture held clearly in mind we then examine one portion of this whole at a low magnification, let us say ten times. We are now studying a detail not for its own isolated sake, but with its relationship to the whole in mind. We can then go on to a closer study of this field within the whole by using another objective of still higher magnification, let us say fifty times. Further and finer analysis of the details within the whole are then made possible by using larger and larger magnifications to the practical limits of the instrument.

We might also think of the material as being classified, not in a straight line series of separated and independent parts which can then be reshuffled in any order, but in terms of 'being contained within,' perhaps, like a nest of boxes. If we call the whole security syndrome a box, then the 14 sub-syndromes are 14 boxes that are contained within it.[1] Within each of these 14 small boxes still other boxes are contained—perhaps 4 in one, 10 in another, 6 in another, etc.

Translating these examples into the terms of syndrome study, we may take the security syndrome and examine it as a whole, that is at level of magnification No. 1. Specifically this means studying the psychological flavor or meaning or aim of the total syndrome as a unity. We may then take one of the 14 sub-syndromes of the security syndrome and study it at what we might call level of magnification No. 2. This sub-syndrome would then be studied in its particular wholeness, in its interdependence with the 13 other sub-syndromes, but always understood as a holistic part of the total security syndrome. As an example we may take the power-submission sub-syndrome in the insecure person. The generally insecure person needs power, but this shows itself in many ways and in many different forms, such as overambition, overaggression, possessiveness, hunger for money, overcompetitiveness, tendency to prejudice and hatred, etc., or as

[1] For more details, see A. H. Maslow, The dynamics of psychological security-insecurity. *Character & Pers.*, 1942, 10, 331–344.

their apparent opposites, *e.g.*, bootlicking, submissiveness, masochistic trends, etc. But these characteristics themselves are obviously also general and may be further analyzed and classified. A study of any of these would be at level No. 3. Let us choose, perhaps, the need for or tendency toward prejudice, of which race prejudice is a good example. If we study this correctly, we study it not in itself or in isolation. We could phrase it more fully by saying that we are studying the tendency to prejudice which is a sub-syndrome of the need for power, which is a sub-syndrome of the general insecurity syndrome. I need not point out that finer and finer studies would take us to level 4, level 5, and so on. We could take for instance one aspect of this particular complexity, let us say the tendency to seize upon differences, *e.g.*, skin color, shape of nose, language spoken, as a means to bolster one's own need for security. This tendency to seize upon differences is organized as a syndrome and can be studied as a syndrome. To be more specific, in this case it would be classified as a sub-sub-sub-sub-syndrome. It is the 5th box in the nest of boxes.

To sum up, such a method of classification, *viz.*, one that is based upon the fundamental concept of 'being contained within' rather than of 'being separate from,' bids fair to give us the clue for which we have been seeking. It allows us to be sophisticated both about particulars and about wholes without falling into either meaningless particularism or vague and useless generality. It is simultaneously synthetic and analytic, and finally it allows us to study uniqueness and commonness simultaneously and effectively. It rejects the dichotomies, the Aristotelian division into class A and class Not-A, and by so doing furnishes us with a theoretically satisfactory principle of classification and analysis.

Concept of syndrome concentration.—If we look for a heuristic criterion by which to differentiate between syndromes and sub-syndromes, we can find it theoretically in the concept of concentration. What is the difference between the natural groupings in the self-esteem syndrome? Conventionality, morality, modesty, and regard for rules were

found to cluster together into a group which could be differ-
entiated from another cluster formed by the characteristics
of self-confidence, poise, unembarrassability, and boldness.
These clusters or sub-syndromes of course correlate with each
other and with self-esteem as a whole. Furthermore, within
each cluster the various elements correlate with one another.
Probably our perception of clustering, the subjective feeling
that various elements 'go together' naturally, would be
reflected in the correlations that would be obtained if we
could get measures of these elements. Probably self-confi-
dence and poise are more closely correlated than are poise
and unconventionality. Perhaps a clustering could mean in
statistical terms high average of intercorrelation between all
the members of the cluster. This average intercorrelation
would presumably be higher than the average of correlation
between members of two different clusters. Supposing we
assume the intracluster average correlation to be $r = .7$, and
the average correlation between members of different clusters
to be $r = .5$, then the new syndrome formed by the merging
of the clusters or sub-syndromes will have an average correla-
tion higher than $r = .5$ and lower than $r = .7$—probably
closer to $r = .6$. As we move from sub-sub-syndromes to
sub-syndromes to syndrome, we may expect that the average
correlation will go down. This change we may call change
in syndrome concentration, and we may reasonably stress the
concept if only because it may furnish us with the working
tool with which we may check our clinical findings.[2]

It follows from the basic assumption of a dynamic psy-
chology that what can and should be correlated are *not*
behaviors *qua* behaviors, but the meanings of behaviors,
e.g., not modest behavior, but the quality of modesty seen
intact in its relations to the rest of the organism. Further-
more it must be recognized that even dynamic variables do

[2] It is the tendency of holistic psychologists to mistrust the correlation technique,
but I feel that this is because the technique happens to have been used in an exclusively
atomistic way rather than because its essential nature conflicts with holistic theory.
Even though, for instance, self-correlations are mistrusted by the average statistician
(as if anything else could be expected in the organism!), they *need* not be if certain
holistic facts are taken into consideration.

not necessarily vary along a single continuum, but may at a certain point break sharply into something completely different. An example of this phenomenon may be found in the effects of hunger for affection. If we range young children in a series from fully accepted to fully rejected, we shall find that as we go down the scale, the children hunger more and more frantically for affection, but as we approach the extreme end of the scale—utter rejection from the earliest days of life —we find not a tremendous yearning for love, but a complete coldness and *lack* of desire for affection.

Finally we must of course use holistic data rather than atomistic data, *i.e.*, not the products of reductive-analysis but of holistic analysis. In this way, single variables or parts may be correlated without doing violence to the unity of the organism. If we are properly cautious about the data which we correlate, and if we temper all statistics with clinical and experimental knowledge, there is no reason why correlation technique should not be highly useful in a holistic methodology.

The extent of the interrelatedness within the organism. In his book on physical Gestalts, Köhler objects to the over-generalization of interrelatedness, even to the extent of not being able to choose between a very general monism and a complete atomism. Accordingly, he stresses not only inter-relatedness within a Gestalt, but also the fact of separateness of Gestalts. For him most of the Gestalts he works with are (relatively) closed systems. He carries his analysis only to the point of analyzing within the Gestalt; he discusses less often the relations between Gestalts, either physical or psychological.

It must be apparent that when we deal with organismic data we have a different situation. Certainly there are almost no closed systems within the organism. Within the organism everything does actually relate with everything else, if only sometimes in the most tenuous and distant fashion. Furthermore the organism taken as a whole has been shown to be related to and to be fundamentally interdependent with the culture, the immediate presence of other people, the particular situation, physical and geographical factors, etc. So far we

may say at least that what Köhler should have done was to restrict his generalization to physical Gestalts and to psychological Gestalts in the phenomenal world, for his strictures certainly do not apply nearly as strongly within the organism.

It is possible to go beyond this minimum statement if we choose to argue about it. Actually a very good case can be made for saying that the whole world is theoretically interrelated. We can find relations of some sort between any part of the universe and any other part if we choose from any of the multitude of relation types that exist. It is only if we wish to be practical, or if we speak in a single realm of discourse rather than in a totality of realms of discourse, that we can assume that systems are relatively independent of one another. For instance, from the psychological point of view universal interrelatedness breaks up because there are parts of the world which are not *psychologically* related to other parts of the universe, even though they may be related chemically, physically, or biologically. Furthermore the interrelatedness of the world might very well be broken up in a completely different fashion by the biologist or chemist or physicist. It seems to me that the best phrasing possible at the moment is that there are relatively closed systems, but that these closed systems are the product in part of the point of view from which we chose to structure the world. From this point of view what is (or what seems to be) a closed system may not be so a year from now because scientific operations may improve enough next year to demonstrate that there is such relationship. If the reply were to be made that we should have to demonstrate actual physical processes obtaining rather than more theoretical relationships between all the parts of the world, then it certainly must be said in reply that the monistic philosophers have never claimed such a universal *physical* interrelatedness but have spoken of many other kinds of interrelatedness. However, since this is not a main point in our exposition, it is not necessary to dwell upon it. It is quite sufficient to point out the phenomenon of universal interrelatedness within the organism.

VI. Relations Between Syndromes

In this area of research we have at least one carefully studied example to offer. Whether this is a paradigm or a special case remain to be discovered by further research.

Quantitatively, that is to say in terms of simple linear correlations, there is a positive but small relationship between the security level and level of self-esteem—r = about .2 or .3. In the area of individual diagnosis in normal people, it is quite clear that these two syndromes are practically independent variables. In certain groups there may be characteristic relations in the two syndromes: *e.g.*, in Jews there is a tendency to be simultaneously high in self-esteem and low in security, while in Catholic women we find often low self-esteem joined with high security. In neurotics both levels are apt to be low.

But more startling than this relationship (or lack of it) in level of the two syndromes is the very close relationship between *level* of security (or self-esteem) and the *quality* of self-esteem (or security). This relationship can be demonstrated most easily by contrasting two individuals both high in self-esteem but at opposite ends of the scale in security. Person A (high self-esteem and high security) tends to express his self-esteem in a very different way from person B (high self-esteem and low security). A, who has both personal strength and love for his fellow-man, will naturally use his strength in a fostering, kindly or protecting fashion. But B, who has equal strength but has with it hate, contempt or fear for his fellow-man, will more probably use his strength to hurt, to dominate, or to assuage his insecurity. His strength must then be a threat to his fellows. Thus we may speak of an insecure quality of high self-esteem, and we may contrast it with a secure quality of high self-esteem. Similarly we may distinguish insecure and secure qualities of low self-esteem, *i.e.*, the masochist or bootlicker on the one hand, the quiet, sweet, or serving, dependent person on the other hand. Similar differences in security-quality correlate with differences in level of self-esteem. For instance, insecure people may be either retiring, withdrawing people or openly hostile

and aggressive people accordingly as they are low or high in
level of self-esteem. Secure people can be either humble or
proud, followers or leaders as their self-esteem levels vary
from low to high.

VII. The Personality Syndromes and Behavior

In a broad fashion, preliminary to more specific analysis,
we can say that the relations between the syndromes and
overt behavior are about as follows: Each act *tends* to be an
expression of the whole integrated personality. This means,
more specifically, that each act tends to be determined by
each and all of the personality syndromes (in addition to
other determinants to be spoken of below). As John Doe
laughs and responds to a joke, we can theoretically tease out
from among the various determinants of this unitary act his
security level, his self-esteem, his energy, his intelligence, etc.
Such a view point is in clear contrast to that brand of trait
theory in which the typical instance is of a single behavior
act determined wholly by a single trait. Our theoretical
statement gets its best exemplification in certain tasks that
are thought of as 'more important,' like artistic creation. In
producing a painting or a concerto, the artist clearly 'puts
himself' completely into the task, and, accordingly, it is an
expression of his whole personality. But such an example,
or, let us say, any creative response to an unstructured
situation—as in the Rorschach test—is at the extreme of the
continuum. At the other end is the isolated, specific act
which has little or no relation to the character structure.
Examples of such are immediate responses to the demands of
a momentary situation (getting out of the way of a truck),
purely habitual, cultural responses which have long since lost
their psychological meaning for most people (getting up when
a lady enters the room), or finally, reflex acts. Such behavior
can tell us little or nothing about the character, for in these
cases it is negligible as a determining factor. Between these
extremes we find all sorts of gradations. There are, for
example, acts which tend to be almost wholly determined by
only one or two syndromes. A particular act of kindness is

more closely related to the security syndrome than to any other. The feeling of modesty is largely determined by self-esteem, and so on.

These facts may raise the question why, if all these types of behavior-syndrome relations exist, should it be said at the outset that behavior is generally determined by all the syndromes?

It is obvious that, by a kind of theoretical requiredness, a holistic theory would start with such a statement, while an atomistic approach would start with the selection of an isolated, discrete behavior, cut away from all its connection to the organism—a sensation or conditioned reflex, for instance. Here it is a problem of 'centering' (from the point of view of which part is the whole to be organized). For atomistic theory the 'simplest' fundamental datum would be a bit of behavior obtained by reductive analysis, *i.e.*, a behavior cut away from all its relationships to the rest of the organism.

Perhaps more to the point is the contention that the first type of syndrome-behavior relationship is more important. Isolated behaviors tend to be on the fringe of life's main concerns. They are isolated simply *because* they are unimportant, *i.e.*, have nothing to do with the main problems, the main answers, or the main goals of the organism. It is quite true that my leg kicks out when the patellar tendon is struck, or that I eat olives with my fingers, or that I cannot eat boiled onions because I was conditioned against them. It is certainly no truer that I have a certain life philosophy, that I love my family, or that I am drawn to do experiments of a certain kind—but the latter situations are far more important.

While it is true that the inner nature of the organism is a determinant of behavior, it is not the only determinant of behavior. The cultural setting in which the organism behaves, and which has already helped to determine the inner nature of the organism, is also a determinant of behavior. Finally another set of determinants of behavior may be lumped under the head of 'the immediate situation.' While

the goals and aims of the behavior are determined by the nature of the organism, and the paths to the goals by the culture, the immediate situation determines the realistic possibilities and impossibilities: which behavior is wise, which not; which partial goals are available and which not; what offers a threat and what offers a possible tool with which the goals may be achieved.

Conceived thus complexly it becomes easy to understand why behavior is not always a very good index of character structure. For if behavior is as much determined by the external situation and by culture as by character, if it is a compromise formation between three sets of forces, it cannot very well be a perfect indicator of any one of them. Again this is a theoretical statement. Practically there are certain techniques [3] whereby we may 'control out' or nullify the influences of culture and situation so that in actual practice, behavior *may* sometimes be a good index of character.

A much higher correlation is found to obtain between character and impulse-to-behavior. Indeed, this correlation is so high that these impulses-to-behavior may themselves be considered part of the syndrome. These are far more free of external and cultural compulsions than are overt behavior acts. We may even go so far as to say that we study behavior only as an index of impulse-to-behavior. If it is a good index, then it is worth studying; if it is not a good index, then it is not worth studying, if the ultimate object of our studying is the understanding of character.

[3] For instance the situation may be controlled out as a determiner of behavior by making it sufficiently vague, as in the various projective tests. Or sometimes the demands of the organism are so overwhelming, as in insanity, that the external world is denied or disregarded and the culture defied. The prime technique for partially ruling out the culture is interview rapport or the psychoanalytic transference. In certain other situations the cultural compulsions may be weakened, as in drunkenness, rage, or other examples of uncontrolled behavior. Again there are many behaviors which culture neglects to regulate, *e.g.*, various subtle, subliminally perceived variations of the culturally determined theme, the so-called expressive movements. Or we may study behavior in relatively uninhibited people, in children in whom cultural compulsions are as yet weak, in animals in which they are almost negligible, or in other societies so that we can rule out cultural influences by contrast. These few examples show that a sophisticated, theoretically sound study of behavior, in contrast to a naive behaviorism, *can* tell us something about inner organization of personality.

VIII. Isolated Responses

Holistic theory does not deny the possibility of isolated impulses or of isolated conditionings or habits which have little or nothing to do with the character structure. To say it in another way, this theory does not claim that all responses are necessarily expressions of the total personality. It is true that there is a tremendous difference in emphasis, as we have already pointed out. But it was also indicated that this discussion involves not the sheer statistical question of the relative frequency of isolated responses and holistic responses, but that it is rather a question of 'centering,' of the point of view from which we shall perceive the organization of personality and of behavior. A more accurate statement of the holistic point of view with regard to isolated responses is not that they are impossible or infrequent, but that they are relatively unimportant. The technique par excellence for producing isolated fears or other emotional responses, or specific, discrete behavior acts is the conditioned response technique. This can still be said even though recent experiments with conditioning show that these phenomena are much more closely related to the whole organism than was previously thought. There is, however, another and perhaps more important kind of isolated response. Where responses have become fully familiar or automatized, then no great recruitment of the capacities of the organism is necessary for these acts and they may therefore occur in a relatively discrete or isolated fashion. Proof of this occurrence is given by the example of several automatisms proceeding simultaneously. I may at the same time walk down the street, whistle a tune, jingle the keys in my pocket and casually step on all the cracks on the sidewalk.

This example can be understood in its fullest significance if we phrase the situation in functional terms. The organism behaves most holistically, most completely as a unit, when it is confronted by a great emergency, an unfamiliar threat, or an unsolved problem which is related to the basic needs of the person. It is then that the recruitment of the capacities in the service of the attempt to solve this problem is most

complete. But as this problem varies from very important to less important, from unfamiliar to familiar, from threatening to non-threatening, from unsolvable to easily solvable, so also does the recruitment of organismic capacities vary from more to less. Ordinarily we may expect that the healthy organism will, so to speak, gird its loins, muster its strength, or recruit its capacities in a relatively economical fashion, *i.e.*, only to the bare extent necessary to solve the problem. It will ordinarily bring up its 'big guns' for big jobs rather than for little ones.

We have so far spoken of the normal healthy organism. In the disturbed organism it is possible to find undesirable tendencies both to unnecessary, general responsiveness and to inefficient, specific responsiveness. Examples are the general anxiety reactions, fixations on partial goals, stimulus-bound behavior, the influence of specific symbolisms or conditionings, and the like.

Exceptions to the internal unity of the syndrome are most frequently due to sheer isolated conditionings or habit, to the unique demand of the momentary situation, or to specific cultural demands or tabus. It has already been shown for those specificities which are not in accordance with the syndrome's inner tendencies that they always have exerted upon them various dynamic forces, arising from the inner nature of the syndrome, which tend continually to force these inconsistent specificities into line with the remainder of the syndrome. Sometimes this pressure is enough to wipe out the inconsistency, but sometimes it is not and the inconsistency persists.   Where it does persist we ought undoubtedly to be able to demonstrate by some *ad hoc* technique that it is more exceptional and more liable to change than other more normal parts of the syndrome, and that where it does tend to change it will be in the prevailing direction of the syndrome.

Thus it becomes apparent that far from neglecting isolated and specific responses, holistic theory gives them their true importance and their true value. It is only from the point of view of general organismic theory that the simultaneous possibility of isolated and holistic responses can be seen as

one of the chief glories of complex structure and one of the highest evolutionary achievements. It should be remembered that in the lower animals the tendency is to respond either too generally or too specifically. It is only the human organism which is capable of responding holistically where this is desirable for the individual's welfare (in the face of important life problems), and of responding specifically when it is possible to save the organism's energy by allowing it to do several things at the same time.

To summarize, the organism tends to respond in a unitary fashion most clearly in the face of important, unsolved, unfamiliar or threatening problems or emergencies. It tends to show its ability to respond specifically most clearly where the problems which it confronts have an easy, familiar, or habitual solution or in which the problems are relatively unimportant for the organism.

IX. Logical and Mathematical Expressions of Syndrome Data

There is not now extant, so far as I know, any mathematic or logic which is suitable for the symbolic expression and manipulation of syndrome data. Such a symbolic system is by no means impossible, for we know that we can construct mathematics or logics to suit our needs. Currently, however, the various logics and mathematical systems that are available are based upon and are expressions of the general-atomistic world view which we have already criticized. My own efforts in this direction are much too feeble to present at this time.

The sharp distinction between 'A' and 'Not-A' introduced by Aristotle as one of the bases of his logic has been carried on to modern logics even where other Aristotelian assumptions have been rejected. So for instance we find in Langer's *Symbolic logic* that this notion, which she describes in terms of complementary classes, is for her one of the basic assumptions which need not be proven but can be taken for granted as common sense. "Every class has a complement; the class and its complement are mutually exclusive and exhaust the universe class between them." [4]

[4] S. Langer, *Symbolic logic*, Boston: Houghton Mifflin, 1937, p. 193.

It must by now be quite obvious that with syndrome data there can be no such sharp cutting away of any part of the data from the whole, or any such sharp division between any single datum and the rest of the syndrome. When we cut 'A' away from the whole, 'A' is no longer 'A,' 'Not-A' is no longer what it was, and certainly simple addition of 'A' and 'Not-A' will not give us back the whole with which we started. Within a syndrome every part of the syndrome overlaps every other part. Cutting away a part is impossible unless we pay no heed to these overlappings. This neglect the psychologist cannot afford. Mutual exclusiveness is possible for data taken in isolation. If they are taken in context, as they must be in psychology, this dichotomizing is quite impossible. It is not, for instance, even conceivable that we could cut away self-esteem behavior from all other behavior, for the very simple reason that there is practically no behavior that is just self-esteem behavior and nothing else.

If we reject this notion of mutual exclusiveness, we call into doubt not only the whole logic which is partially based upon it, but also most of the systems of mathematics with which we are familiar. Most extant mathematics and logic (perhaps all) deal with a world which is a collection of mutually exclusive entities, like a pile of apples. Separating one apple from the rest of the pile changes neither the essential nature of the apple, nor the essential nature of the pile. With the organism it is quite different. Cutting away an organ changes the whole organism as well as the part that was cut away.

Another example may be found in the basic arithmetical procedures of addition, subtraction, multiplication, and division. These are all operations that clearly assume atomistic data. Adding an apple to another apple is possible because the nature of apples permits this. The case is different with personality. If we have two people who have high self-esteem and are insecure and we then make one of these people more secure ('add' security), we then have one person who probably tends to be cooperative and another who tends to be a tyrant. The high self-esteem in one personality does not have the

same quality as the high self-esteem in the other. In the person to whom security has been added there are two changes, not just one. Not only did he acquire security, but the quality of the self-esteem changed, merely by being conjoined with high security. This is a labored example, but it is the closest that we can come to conceiving of anything like the additive processes in personality.

Apparently traditional mathematics and logic, in spite of their unlimited possibilities, seem actually to be handmaidens in the service of an atomistic, mechanical view of the world.

It seems even possible to say that mathematics is lagging behind modern physical sciences in its acceptance of dynamic, holistic theory. Essential changes in the nature of physical theory are made, not by changing the essential nature of mathematics, but by stretching its use, by doing tricks with it, by leaving its essentially static nature unchanged as much as possible. These changes can be made only by making various 'as if' assumptions. A good example is found in calculus which purports to deal with motion and change, but does this only by making the change into a succession of static states. The area under a curve is measured by splitting it into a series of oblongs. Curves themselves are treated 'as if' they were polygonal figures with very small sides. That this has been a legitimate procedure with which we can have no ultimate quarrel is proven by the fact that the calculus works and is a highly useful instrument. But what is not legitimate is to forget that it works because of a series of assumptions, of dodges or tricks, of 'as if' assumptions which clearly do not deal with the phenomenal world as psychological studies do.

The following quotation is an illustration of our contention that mathematics tends to be static and atomistic. So far as I know, its purport has not been challenged by other mathematicians.

But had we not previously declared quite fervently that we live in a motionless world? And had we not shown at great length, by employing the paradoxes of Zeno, that motion is impossible, that a flying arrow is actually at rest? To what shall we ascribe this apparent reversal of position?

Moreover, if each new mathematical invention rests upon the old-established foundations, how is it possible to extract from the theories of static algebra and static geometry a new mathematics capable of solving problems involving dynamic entities?

As to the first, there has been no reversal of viewpoint. We are still firmly entrenched in the belief that this is a world in which motion as well as change are special cases of the state of rest. There is no state of change, if change implies a state qualitatively different from rest; that which we distinguish as change is merely, as we once indicated, a succession of many different static images perceived in comparatively short intervals of time. . . .

Intuitively convinced that there is continuity in the behavior of a moving body, since we do not actually see the flying arrow pass through every point on its flight, there is an overwhelming instinct to abstract the idea of motion as something essentially different from rest. But this abstraction results from physiological and psychological limitations; it is in no way justified by logical analysis. Motion is a correlation of position with time. Change is merely another name for *function*, another aspect of that same correlation.

For the rest, the calculus, as an offspring of geometry and algebra, belongs to a static family and has acquired no character- istics not already possessed by its parents. Mutations are not possible in mathematics. Thus, inevitably, the calculus has the same static properties as the multiplication table and the geometry of Euclid. The calculus is but another interpretation, although it must be admitted an ingenious one, of this unmoving world.[5]

Let us say again that there are two ways of looking at elements. For instance blushing can be blushing *per se* (a reductive element) or it can be blushing in a context (a holistic element). The former involves a kind of 'as if' assumption, "as if it were all alone in the world and had no relation to the rest of the world." This is a formal abstraction and in certain scientific areas may be quite useful. In any case certainly no harm can be done by the abstraction so long as it is remembered that it *is* a formal abstraction. Trouble arises only when the mathematician or logician or scientist forgets that he is doing something artificial when he talks about blushing *per se*, for certainly he would admit that there is in the real world no such thing as blushing without a

[5] E. Kasner, & J. Newman, *Mathematics and the imagination*, New York: Simon, Schuster, 1940. Pp. 301–304.

human being to blush, something to blush about, etc. This artificial habit of abstraction, or working with reductive elements, has worked so well and has become so ingrained a habit that the abstractors and the reducers are apt to be amazed at anyone who denies the empirical or phenomenal validity of these habits. By smooth stages they convince themselves that this is the way in which the world is actually constructed, and they find it easy to forget that even though it is useful it is still artificial, conventionalized, hypothetical —in a word, that it is a man-made system which is imposed upon an interconnected world in flux. These peculiar hypotheses about the world have the right to fly in the face of common sense only for the sake of demonstrated convenience. When they are no longer convenient, or when they become hindrances, they must be dropped. It is dangerous to see in the world what we have put into it rather than what is actually there. Let us say this flatly. Atomistic mathematics or logic is, in a certain sense, a theory about the world, and any description of it in terms of this theory the psychologist may reject as unsuited to his purposes. It is clearly necessary for methodological thinkers to proceed to the creation of logical and mathematical systems that are more closely in accord with the nature of the world of modern science.[6]

[6] It is possible to extend these remarks to the English language itself. This too tends to reflect the atomistic world theory of our culture. It is not to be wondered at that in describing syndrome data and syndrome laws we must resort to the most outlandish analogies, figures of speech and various other twistings and turnings. We have the conjunction 'and' to express the joining of two discrete entities, but we have no conjunction to express the joining of two entities which are not discrete and which when joined form a unit and not a duality. The only substitute that I can think of for this basic conjunction is the clumsy one 'structured with.' There are other languages which are more sympathetic to a holistic, dynamic world view. In my opinion the agglutinative languages are more adequate to reflect the holistic world than is the English language. Another point is that our language organizes the world, as most logicians and mathematicians do, into elements and relationships, matter and things done to matter. Nouns are treated as if they were matter and verbs as if they were actions done by matter to matter. Adjectives describe more accurately the kind of matter and adverbs describe more accurately the kind of action. A holistic dynamic view makes no such sharp dichotomy. It is to be expected that we shall have to proceed in a Procrustean fashion to express such data in the English language.

THE DESCRIPTION OF PERSONALITY.
I. FOUNDATIONS OF TRAIT MEASUREMENT

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I. The Present Barrier in Descriptive Psychology

In spite of the impressive accumulation of data on personality in relation both to environment and heredity, advances in exactness of prediction or depth of theoretical understanding have shown a distinct lull in the past few years. It is as if the forces of research, while advancing by sheer pressure of numbers on the flanks, have been held up in the centre by an impassable bridgehead. The bridgehead controls transition from the habit of defining personality in the vague terms of popular speech to the practice of using exact and measurable variables based on clear theoretical conceptions. The present paper is a contribution to this transition.

Measurement of a kind—leading even to impressive statistical treatments—admittedly exists. But a closer examination shows the figures to be specious and lacking in some or all of the characteristics required of true mensuration. The proponents of measurement in this somewhat shadowy world of personality qualities stake their defence on the well known dictum of Thorndike (29) that "whatever exists, exists in some quantity, and can therefore ultimately be measured." Unfortunately the optimism engendered in some psychometrists by this excellent statement blinds them to certain basic conditions of measurement, notably to the rule of elementary algebra that added units must be of the same kind. Therefore our first step must be to emphasize that measurement can only follow upon the correct recognition and definition of qualitative characters, *i.e.*, upon advances in descriptive psychology.

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Because of the demoralizing disappointments which have beset the prosecution of descriptive psychology—in attempting to describe the stream of consciousness, in 'faculty' psychology, in the *Gestalt* and the *Geisteswissenschaftlich* approaches, and even in some aspects of factor analysis (12, 18, 25)—it seems necessary to rally effort at this point by reiterating the obvious truth that personality research depends entirely on the soundness of personality description and measurement. For, stripped of particularities, all research methods consist of just this: measurement of personality at a given moment, followed by lapse of time or application of certain influences, and finally remeasurement. Consequently the ability to deal with morphology is a prerequisite in inquiring about growth and function. This is but another way of saying that the genotype can be understood only through studying the phenotype. Or again, looking at the matter from the mathematical standard of degree of accuracy, it may be said that the precision of predictions about the growth and dynamic interactions of traits is limited by the exactness with which personality can be described and measured in cross section, *i.e.*, statically.

This primary importance of description is not always adequately realized or accepted. For the starkness of the above methodological form is commonly hidden by complications. And occasionally the canon that description is primary may seem completely flouted, as when one observes the great progress achieved by psycho-analysis in understanding dynamics, in spite of its having been singularly negligent of description and measurement. But the contradiction is only apparent, for in fact the technical level attained by psychiatry in sheer description of psychotic and neurotic syndromes far exceeded that of psychology in detecting and classifying individual differences in normal subjects. Freud was able to clarify the mechanisms of hysteria because he could recognize when the hysterical syndrome became intensified or reduced; whereas the psychologist found out little about the origins of, say, suggestibility in children, because he was not able to delimit the trait of suggestibility. The law that nosology precedes etiology is not easily broken.

II. Present Frontiers in the Progress of Trait Definition

The contribution of this paper to the problem of personality description is, specifically, the propounding of a new hypothesis concerning the nature of traits and the description, in outline, of a methodology for the empirical determination of trait unities. It thus prepares the ground for trait measurement. The paper follows an earlier article (8) tentatively clarifying certain personality concepts, and precedes, as a theoretical introduction, two experimental researches (9, 10).

Reviewing the present position of trait theory, no one can doubt that the most widely accepted hypothesis, especially in educational, clinical, industrial and other branches of applied psychology—in which rating scales flourish—is that personality can be described in terms of discrete if not independent traits. By adopting the creed that traits are single functional entities, alike and comparable for different individuals, the psychometrist is enabled to pursue, without any sense of sin, the practice of converting merely qualitative into scientifically quantitative description. Factor analysis also subscribes to this hypothesis, but it can claim that its sins of assumption, if they exist, are small ones; for its major independent factor traits are built empirically on a foundation of many minor traits, each of which, as a trait, is so narrow and specific that no great assumption is made in presuming its unitariness.

This facile theoretical concession to the practical convenience of applied psychology has not, however, remained unchallenged. Psychologists of many different backgrounds have protested that (1, 32) independent traits do not represent the true structure of personality or have questioned the alleged range and consistency of the traits employed (20, 27, 28). But the urgencies of practice seem to have brushed these theoretical objections aside as so much hair-splitting. Indeed, as often happens, the sheer volume of applied psychological publication seems to have crowded more subtle argument from the field of discourse, to the extent that a casual reader might easily gain the impression that the critics,

rather than the proponents, of atomic traits, are intellectually in hiding.

Those who are concerned to go beyond the use of traits merely as counters, who wish to examine them more closely and who insist that realism must have priority over convenience or theoretical systematization, we may call, for this discussion, 'naturalists.' As representative of naturalism and of the advances that can be made by shrewd observation and insight, we may take the position of Allport. He begins by recording the important phenomenological difference between unique traits and common traits, saying (1, p. 297), "Strictly speaking no two persons ever have precisely the same trait" for, "What else could be expected in view of the unique hereditary endowment, the differing developmental history, and the never repeated external influences that determine each personality?" On the other hand, he argues, heredity and environment are sufficiently alike for the majority of people in one culture to give substantially the same form to the behavior of mature adults in many fields of quantitative individual difference, e.g., in dominant-submissive behavior, or radicalism-conservatism. In this way arise traits which, by contrast to the above, may be called 'common traits.'

The view that all traits are essentially unique, but that uniqueness approaches asymptotically the state of commonness, is an indisputable conclusion alike of common sense and clinical observation.[1] By this view common traits would be measurable, in terms of a common direction and common units, but unique traits would not. Actually the mathematical psychologist can claim that unique traits are measurable in units unique to the individual, but this is rather a Pyrrhic conquest for measurement.

Allport's second division of traits follows one made by Stern (26) between driving traits (*Richtungsdispositionen*) and instrumental traits (*Rüstungsdispositionen*), which traits, for

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[1] In agreeing with this view the present writer does not also subscribe to Allport's criticism that factor analysis is incompatible with giving uniqueness to the individual. As Guilford (14) very lucidly argues, the uniqueness exists in the combination of common factors. There are uniquenesses in Allport's sense, nevertheless, which escape representation by common factors.

conformity with modern terminology, are called respectively motivational and stylistic traits. The nature of the unity underlying these traits differs, the former being teleonomic (*i.e.*, resident in common teleology, dynamic root, purpose or goal of the parts) and the latter stylistic (*i.e.*, resident in a likeness of the parts with respect to the *way* in which an end goal—of any kind—is striven for).

If space permitted, a preliminary review of the present positions in regard to the nature of traits would illustrate also the comparatively naive orthodox nomothetic view (3, 18), the view that there are not traits but only specific habits, a position closely associated with stimulus-response psychology (28, 32), the Gestalt view (4, 19), and the empirical statistical view that a trait exists where the intercorrelations of trait elements form a cluster of high values, *i.e.*, when there is an 'operational unity' (13, 15, 31). The utility of these views will be discussed explicitly or by implication below.

III. The Nature of Trait Unities

At this point we propose to describe the kinds of unity or integralness which, on a wide variety of psychological grounds, can be argued to constitute the possible binding unities in the elements of behavior. We shall discuss both the conceivable kinds of trait unity and the kinds which are actually implicit in some thousands [2] of trait terms in general use. Finally we shall ask which of these unities are real and which fictitious or misleading. In the ensuing section we shall try to determine by what methods of empirical observation these various types of true unity may be detected.

Any given unity is constituted by an integration of smaller behavior elements, which we will call 'trait elements' and the atomic nature of which, for the moment, may be assumed, by reason of their being the result of repeated subdivision. The words trait and syndrome are, therefore, used interchangeably

[2] The list in question is that of Allport and Odbert (3) compiled from Webster's dictionary, and the present comments arise from a detailed study of it in connection with a further research (9).

here, both being larger aggregates of putative elementary pieces of behavior.

The trait unity classification at which we arrive has six categories, as follows.

1. *Dynamic* (*teleological, teleonomic, motivational*) *unities.*— This form of unity is Allport's and Stern's directive unity, in which the parts are integrated by the fact that they all lie on the path to some one goal of the organism. They have a common purpose. A very appreciable proportion of the more important traits in trait lists [3] fall in this category, including the definitely 'dispositional' (21) traits, *e.g.*, timidity, amorousness, assertiveness, acquisitiveness.

It is at first surprising that although language is well supplied with trait terms corresponding to drives with a relatively generalized goal object, such as needs, propensities or ergs of any kind (8), it is practically mute with regard to terms for the dynamic, teleological unities centering upon special, immediate, local objects of the environment. These latter unities are sentiments, attitudes and complexes acquired in the individual life of the subject and may be covered by the categorical label 'metanergs' (8). It may be that these dynamic attachments of mixed ergic origin are generally too narrow, transient or unimportant, either from the standpoint of the single personality or of society, to earn a denomination. Yet to decide that they can be thus dismissed because language ignores them is too temptingly easy, and, to judge by clinical experience, quite fallacious. It is most likely that their great variety alone accounts for the failure of language to crystallize terms. For where we find a sentiment unity, or even a comparatively superficial attitude, that has some claim to being a common trait there is usually a term for its presence, *e.g.*, patriotic, domestic, communist, fatherly.

This discovery that traits are to be found corresponding

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[3] Murray's scheme (22) for rating individuals according to 'needs' is based on just such an assumption of unity of behavioral intention or goal. The methodology suggested later in this article should be capable of revealing the exact boundaries of these important intentional unit systems and of deciding, for example, whether Murray's fifty-one needs, or McDougall's sixteen propensities, or some other approach to ergs most correctly depicts personality structure.

to sentiments as well as to native ergs or propensities, reveals that there are two senses in which the term 'dynamic trait' is actually employed.   Rating an individual for an ergic trait, *e.g.*, a need, requires an act of abstraction, which infers the strength of a construct from analysis of a number of actual sentiment manifestations.   Certain sentiments may be, among other things, manifestations of, say, acquisitiveness and the observer sums them from this aspect.   The unity, in short, is imposed by the observer—though along lines indicated by biological and other considerations as to the nature of this potential structure, the erg.   It also resides in the quality of satisfaction, as introspected by the subject.

On the other hand, the *actual* unities in the dynamic structure of personality, the sentiments, formed by conditioning etc., enjoying some degree of functional autonomy in their attachment to particular objects, are derived from diverse ergic roots.   Without conditioning and all the environmentally determined processes of sublimation and aim-inhibition, these latter structures would not exist.   Their existence makes it necessary to speak of two kinds of dynamic trait unities: (1) Those unified about a real object.   They may be called *metanergic* dynamic traits; (2) those unified about a perhaps never attained and frequently never conceptualized biological goal, and which we may call *ergic* dynamic trait unities.

In view of the uniqueness of sentiments and of their ergic constitution, it might be contended that psychology, like language, should concern itself little with metanergic dynamic unities.   But the clinician may reply that individual differences in ergic endowment are less important than differences in manner of cathexis or object attachment of these drives.

2. *Social mould (environmental demand) unities.*[4]—Many trait terms point to a unity from the standpoint of society's needs or as occasioned by the pressure of the environment rather than from that of the individual's ergic goals.   For

[4] 'Environmental mould' would be more correct, because more comprehensive. But it is more cumbersome, and the great majority of such traits seem to be socially rather than physically moulded.

example, society needs, in certain fields of the individual's total activities, the quality of honesty. Educational pressure is brought to bear creating a number of specific habits, each of which is directed to the common social purpose of ensuring public honesty. These behavior elements may be attached to quite diverse dynamic systems within the individual, *e.g.*, to affection, fear or self assertion, to patriotism or filial loyalty. Such unities will arise when the press of the environment tends to act with equal strength with respect to all the elements demanded, as in a mould or die press. Thus with respect to honesty, if the individual has lived in a good environment he will have been strongly pressed for all kinds of honest behavior, whereas if one kind of honest behavior has been neglected in his upbringing it will be likely that all the rest have had an equally faint imprint. Consequently, as between individuals, the trait elements will vary together, and in one individual, will be united by common origin and fate, notably common rate of extinction, depending on common age of formation.

Traits which are largely social-environmental mould unities are conscientiousness, courageousness, trustworthyness, tactfulness, cynicalness, charm, superciliousness, humaneness, selfishness, general inhibitedness (one form of introversion?) religiosity, etc. It may be generalized that *they are largely traits for which there are opposites in the dictionary*, whereas this is not true of dynamic traits, at least of the ergic kind. Except where the opposites represent reactions against society's pressure they are generally merely colorless refractory or inertia states, *e.g.*, cowardly, untrustworthy, lacking in charm.

Most social mould patterns appear to have a name, but not all socially evaluated forms of behavior covered by a name need issue in a real pattern in behavior, as the fifth category (below) shows. Social moulding and conscious social recognition and labelling are not the same. The unity of the trait elements is one of common variation, the conscious social purpose being a common but not an invariable or basic character of the unity. It is conceivable and likely that many social and environmental mould traits exist for which

there is no name or social recognition, *e.g.*, a set of acquired habits may distinguish the good pedestrian from the bad pedestrian, or the citizen experienced in undergoing bombing from the inexperienced. With traits that develop through moral and educational exhortation, however, it seems likely that a better unity of the elements will arise when a trait name exists and the social need which it meets is explicit, conscious and educationally supported. Even then the unity may nevertheless be of a low order, because the term is differently understood by different educators and because punishment and reward do not always act in the area perceived only by the educator. Punctuality, for example, may be conceived to apply to different patterns of situation in different social groups (**13**), and even in the same social group the individual may be most heavily punished for something which never entered into the concept, *e.g.*, not appearing punctually for dental check-ups. On the whole, therefore, the common possession (covariation) of elements in the unity will not be great (**13, 15**). Incidentally we may note that in so far as social mould traits form part of a single culture pattern they will also be related (*i.e.*, not statistically independent) in the individual, *e.g.*, if a culture demands of the good citizen, among other things, politeness and courage, the elements of each may show nearly as close an agreement with those of the other as among themselves.

A few social mould unities will also be quite highly developed dynamic unities of the second, acquired type (sentiments), but, as indicated in the last paragraph, there will be some social pressures which are the same for all people yet which are never verbalized enough to be conscious sentiments or sufficiently centered on a single object to be unconscious sentiments. For some purposes the making of a distinction between conscious and unconscious social mould unities might be useful: the former would commonly have some degree of dynamic organization as sentiments; the latter would be collections of discrete attitudes and habits.

3. *Constitutional, non-dynamic (temperamental) unities.*— There exist behavior elements united in a common variation

and common fate because they spring from some unitary constitutional endowment of the organism, which, however, is not of a dynamic teleological nature. The endowment concerned is one of physical energy, nervous sensitivity, fatigability, reaction-time speed, susceptibility to inhibition, or some other unitary character not imposed by the environment. Although the unitary character does not reside in the environment the behavior elements are, of course, an interaction between an heredity and an environment, and in some cases the role of the environment is evident in provoking the emergence of the pattern even though it plays little or no part in deciding the nature of the pattern, *e.g.*, in hereditary psychotic unities, as when manic depressive disorder is precipitated by environmental stress or epilepsy by a tumor. In a true constitutional trait, environment, in so far as it affects it, affects it as a whole. Prolonged excessive demands on the part of environment might reduce the trait of 'energeticness,' or disease (*e.g.*, myxedema) might affect general speed of response. On the other hand the 'general inhibition' or 'introversion' produced by an unduly punishing environment should be distinguishable from that general inhibition and introversion due to constitutional sensitivity by the fact that the former has a restricted pattern, depending on the elements of the environmental mould which have been repressive. Furthermore the constitutional trait will be more common and similar for all individuals, in so far as human genetic endowment is more uniform than are cultural patterns and accidents of upbringing.

Contingently the following may be thought of as illustrating constitutional unities: excitability, extraversion-introversion (in one usage), intelligence, manual dexterity, general inhibitableness, general emotionality, somatotonia, goodness of retention, and some psychotic and neurotic syndromes.

4. *Co-nascent (developmental, emergent) unities.*—A basis for classification which naturally occurs to one in thinking over the principal grounds for the classification of living objects and their characters is that of age and development. A trait would then be considered unitary if its parts emerge

at the same time, ontogenetically or phylogenetically. Incidentally the word co-nascent is employed because both 'developmental' and 'genetic' would be misleading. All traits develop, and genetic is best used as synonymous with 'hereditary.' Actual trait names of this class are to be found, though they are rare, somewhat anomalous and not particularly important. We find 'infantile,' 'adolescent,' 'mature' and 'senile.' 'Beastly' and 'monkey-like' perhaps illustrate the phylogenetic usage.

Syndromes of this type will be found to be in essence either of a constitutional or of an environmental mould type (almost invariably lacking dynamic unity), but possessing the added peculiarity that their parts emerge together. This historical 'accident' may be an aid in calling attention to the unity, but it does not add anything to its structural nature. Probably the greater proportion are unities of a constitutional variety, *i.e.*, syndromes determined by the constitution of the organism, *e.g.*, 'senility.' Some, however, *e.g.*, adolescent, as Mead's studies of adolescence in different cultures show, are combined products of the environmental mould and of environmental provocation of patterns intrinsically constitutional. There is thus no reason for considering con-nascence as an additional *essential* category.

5. *Logical* (*stylistic*,[5] *evaluative, semantic*) *unities.*—This category is suggested by Allport's stylistic trait (1), but extends much more widely. He contrasted such traits with dynamic traits, for the former describe the individual's characteristic way of working toward some goal whereas the latter are labelled by the goal itself. Examples are: forceful, polite, dainty, gruff, eloquent, direct, debonnaire and ceremonious.

[5] In certain cases the stylistic type of logical trait *may* be or may have been part of a dynamic unity. A trait such as politeness may have begun as an instrumental habit in a dynamic trait. And, since functional autonomy is never complete, even in the aged and rigid, the persistence of the style indicates some reinforcement and reward, however obscure. Stylistic traits in Allport's sense in fact would be variously classified here. Some are dynamic, some a result of a pattern of early training (social mould) and some may even be constitutional. When they are dynamic, however, they are mainly brief instrumental habits and skills which can be variously employed in the service of major traits. The majority of Allport's instances, however, seem to be logically bounded, in the above sense.

These traits have no claim to functional unity. Primarily the unity is an evaluative one, existing in the mind of the observer. Now this is only a special case of those perceived unities in which the classification of the elements together is performed on some purely logical basis, divorced from any intrinsic, functional unity. The logical bases may be very varied. The elements may be classified together because they have the same style, speed, social effect or purpose, moral valuation, aesthetic character, etc., etc. If they have the same social effect or purpose, *e.g.*, evoking friendly responses, performing services with respect to machinery, they may secondarily, as a result of the social response to them, acquire some functional unity, in which case they are also true unities—social mould unities—but this is comparatively rare. However, practically any basis of abstraction in the mind of the observer is likely in some special situation to have some practical use, so that every logical category is potentially a functional category. This point is developed below.

Examples of this wider logical unity are very common among trait terms. Many of the earlier (and, unfortunately, some of the later) 'special aptitude' categories in vocational guidance are purely evaluative, *e.g.*, clerical aptitude, social intelligence, and have no demonstrated functional, psychological unity in any of the above three senses. Personality traits such as trustworthy, obnoxious, decent, formidable, exhausting belong in this category.

An extreme variety of evaluative unity, in which even logical coherence is lost, occurs with what may be called semantic or etymological unity. Here the elements are bound by nothing more than the social habit of referring to them by the same name. This is the least excusable of the hypostatizations which some psychological critics so strongly deprecate. Pure instances are rare, but some degree of arbitrary, verbally-imposed unity may be suspected in such traits as lady-like, chauvinistic, Falstaffian, Prussian, Babbittlike and bourgeois. The origins of these counterfeits seem to lie in a term having survived an historically real type, or in

etymological accidents, or in the far-fetched metaphorical usage of a term accurate enough in its own context, *e.g.*, 'musty,' 'flabby,' 'asthenic,' 'acrid,' 'lionlike.' As Zola's description of the Parisian woman or Kipling's description of the empire builder is said to have helped create the type, so these fictions may conceivably also provoke corresponding real functional unities (social mould type), in this way resembling the true logical trait categories.

6. *Miscellaneous forms of trait unity.*—The kinds of unity according to which trait elements *could* be grouped in unitary traits are presumably unlimited. Classification could be made, for example, according to such bizarre features as the relative frequency of use of various motor organs or according to the alphabetical order of the stimuli which evoke the trait elements. Our task has been, however, to discover the senses in which the term trait is already implied by its dictionary examples, and to ask which of these or other senses are psychologically meaningful or desirable. The existing usages which remain to be surveyed are few or unimportant. Chief among them is the view that traits might be named by the stimulus situations which evoke them or the kind of response habit by which they are expressed.

Like most attempts at scientific description in terms of stimulus and response—neglecting that third class of variable, the state of the organism and its drives—this view seems to have fitted reality too poorly to have produced verbal symbols capable of standing the test of usage. One may track down such apparent instances as 'alcoholic,' 'music-loving,' 'mechanically-minded,' 'sailorly' or 'fond of gardening,' in which the stimulus situation labels the trait. But closer examination shows one is being deceived by the ambiguity which constantly dogs the term 'stimulus,' giving it at one time the sense of an initiating situation and at another the sense of a goal symbol. In so far as it is the latter, the labelling by a stimulus is not different from that labelling by goal which is characteristic of all dynamic traits. Indeed the above instances are seen to be metanergic (sentiment) dynamic unities.

These instances of relatively specific sentiment systems invite a discussion of the view that there are no 'traits' but only specific habits. As a representative of this view we may take Weiss, who, criticizing the habitual hypostatization of the nomothetic psychologists, says, regarding manifestations of the alleged trait 'benevolence': "From the sensorimotor standpoint these actions are all different, but because they have the same biosocial effect they may be classified as having . . . equivalence . . ." (32, p. 305). We argue, in opposition to his insistence on the sensorimotor standpoint, that it has been found of no use where traits are concerned. The widest variety of sensorimotor connections may be employed in the service of a single trait.

Occasionally one encounters instrumental sensorimotor habit systems, *e.g.*, auto-driving, footballing, verbal eloquence, chain smoking, or others of a more individual and unverbalizable kind, such as have been called 'actones,' skills and, sometimes, attitudes, which have reached such a degree of development that they are important enough to include in the category of metanergic dynamic traits. They are, however, so frequently attached as servants to some larger sentiment or complex integrate, or available for interchangeable service in the interests of a variety of sentiments and purposes, that they can scarcely rank as dynamic traits in themselves and belong rather in the realm of motor skills.[6]

The last conceivable criterion of trait unity which we have to consider is by no means the least, and has been left till last because it requires extensive discussion, running through the rest of this article. It is embodied in the view that the only real unity of trait elements lies in their correlating positively together in a 'cluster' or mathematical 'factor.' This empirical view has been uncompromisingly stated by Thurstone, in an article criticizing "The traditional methods of dealing with these complexities (trait structures)" because they "have

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[6] The restricted trait spoken of here seems to be identifiable with Hull's (16) 'habit family hierarchy' defined as "a group of two or more habit sequences all of which may be initiated by a particular stimulus and terminated by a particular reaction."

been speculative, bibliographical or merely literary in character" (30, p. 2).

The view that traits are only to be discovered by empirical studies of covariance is strongly maintained and developed in all the following discussion. But it is equally strongly disputed that the definition of a trait merely as a mathematical factor or, still more inadequately, as a simple cluster of correlating elements, is defensible.

Our solution to the problem of traits is thus to recognize that several—basically three—distinct types of trait unity exist. Now in the past much discussion of the nature of traits and the methods whereby they might be investigated has been dogged by the misleading and often unconscious assumption that they are all of one nature, or that only one kind of unity matters. A consideration of something more tangible, say agricultural implements, which may be classified according to color, weight, cost, place of manufacture, agricultural function etc., each grouping having some utility and some groupings being more universally functional than others, will suffice to remind one that organization is relative to a purpose.

For most clinical work dynamic traits and constitutional traits seem to be of primary importance. In education there is relatively more attention to social mould traits. In personnel selection and vocational guidance, as already pointed out, the logical, evaluative trait has enjoyed great popularity. Such traits, e.g., clerical aptitude, consist of functionally unrelated elements of behavior which happen to have in common certain social or physical effects. But even the logical unity may be false, for the trait in question is often philosophically hard to define. Does honesty, for example, include aesthetic and intellectual honesty? Each psychometrist draws his own 'logical' boundaries for the trait he is testing, turning applied psychology into a Tower of Babel.

Even if this difficulty were overcome, through agreement to define traits by fiat, the logical type of trait unity would remain an undesirable and unreal one. In this matter the

pure psychologist is at the moment being more practical than the practical psychologist who favors such traits. For although a test of, say, sociability, leadership or clerical aptitude, made up of a farrago of unrelated behavior elements falling in anyone of these fields, may tell us about the individual's behavior, from the evaluative standpoint of society or vocation (as the psychologist sees it), at that particular moment in that particular field, only measures of the real underlying unities of a dynamic, constitutional or social mould type can enable us to generalize about the person's performance in modified circumstances or to predict performance in years to come.

From this discussion it follows that there are only three kinds of unity which are truly intrinsic and functional (in respect to one or more senses of functional), namely, (1) dynamic trait unities, (2) constitutional trait unities, (3) social mould trait unities. For some social purposes it is useful also to consider logical trait unities.

IV. Basic Methods in Determining Trait Unities: The Case of Dynamic Traits

Accepting the viewpoint that the task of personality research is to investigate not one, but several kinds of trait unity, by the combination of examples of which the personality can be described, we may now ask how these unities are to be experimentally discovered. That is to say, by what methods may the boundaries of dynamic, constitutional and social mould traits be explored. (The boundaries of logical trait unities can, of course, be arbitrarily fixed.)

Let us admit at the beginning that the methods by which psychology—behavioristic psychology—*establishes* causal or functional connections are in essence no different from those of other sciences. Introspection, as involved for example in psychoanalysis and a great variety of current methods employing naive verbal report, may give guidance, but ultimately it has to be stiffened by objective proof. The methodology of psychology, in so far as it is concerned with establishing causal connections or functional unities, has to

proceed by the universal method of discovering *covariation and inevitable sequence*, among observed events. This fact is frequently overlooked but seldom contested: it is seriously contested only by the view that 'equivalence' of behavior elements can be established by intuition and observation of similarity of form, a view which will be discussed later.

The objective methods of establishing trait unities which have so far received approval are the calculation of correlations, the application of factor analysis or the use of analysis of variance. All these are methods which look for common variations in trait variables in the field of individual differences. The notion has been implicit, in such correlation studies, that the method is capable of detecting any kind of trait unity, and it has been suggested that it be applied to discovering dynamic unities and deciding, for example, whether the massive drives posited by Freud, or the smaller propensity units defined by McDougall, or the still finer differentiations represented by Murray's needs, give the most accurate account of the ergic structure of personality.

This suggestion seems to overlook the important principle that different kinds of trait unity will yield different kinds of correlation pattern. Dynamic traits, indeed, stand in a special position in this respect, and we may well approach the general problem of deducing trait structure from correlation data by devoting this first section to the special case of dynamic traits.

The basic generalization we wish to stress here is that, among individuals possessed of equal endowment in a particular drive, different manifestations will vary inversely and be negatively rather than positively correlated. The situation may be explored more fully by means of Fig. 1, in which we take a minimum population of two persons, possessing differing endowments in the basic erg (in this case sex drive) and differing amounts of investment in different manifestations. For clarity of discussion it is necessary, further, to introduce the notion of 'levels' of expression, representing stages in ontogenetic development and degrees of arborization in the dynamic structure. Thus in this case there is first a break between

AREAS OF BEHAVIOR
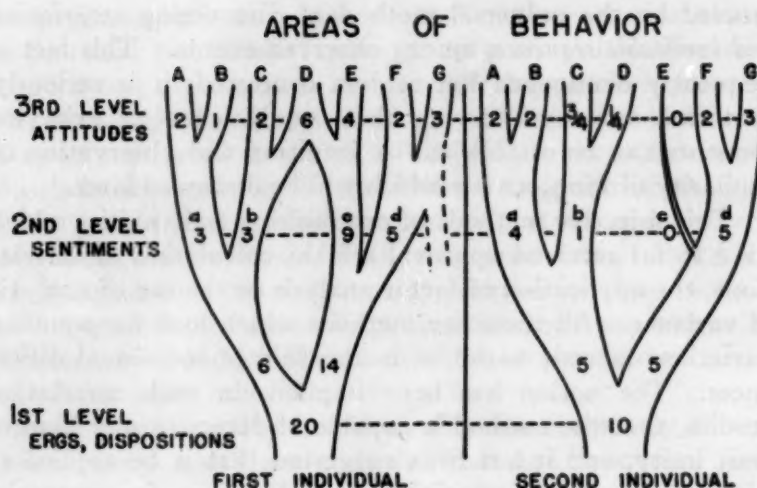
FIG. 1.

adult and infantile sexuality and then various levels of
sublimation or complexity of expression of adult sexuality.
The strength of the investments is indicated by numbers [7]
which, at all levels, add up to the same quantity—that of the
individual's total endowment, as indicated at the root.

It is obvious that in this, or any larger population, the

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[7] The use of quantities here provokes discussion again of the meaning of common
and unique traits, for it may well be argued that, especially in dynamic traits, the finer
ramifications are unique. Our whole purpose is to measure common traits, but we
must admit that the small *trait elements* out of which, by correlation, the larger common
traits may be constructed, are themselves unique and measurable only in logical
(metric) not population (normative) units. There are theoretically two ways of
handling the measurement of these trait elements. We may make our small logical
categories so fine and numerous that even the most bizarre form of behavior will find
a category in which its presence or absence can be recorded. Or we may retain cate-
gories of reasonable size and record qualitative as well as quantitative variations in
each, expressing the qualitative variations as directions of vector quantities. The
latter seems impracticable, for we have no means of making a combined factor analysis
of directions and quantities.

This consideration reminds one that all trait forms eventually depend on measure-
ments in logical trait categories. All traits are relations between organism and
environment. They do not reside only in the organism. Since the cultural environ-
ment, and to a lesser extent the heredity of organisms, slowly change, the functional
unity of a given common trait is not eternal. The common traits of an ancient
Egyptian might not be measurable in terms of common traits established today.
But the logical trait categories of the trait elements are (or can be) permanent. They
are the dust from which the organic, functional unities are built and to which they
return.

manifestations a + b on the one hand and the manifestations c + d on the other, will tend to show a negative correlation because they are alternate and complementary manifestations of the individual's total endowment. They will also tend to show a positive correlation because they spring from the same common endowment. If a factor analysis is made of the correlations of the A, B, C, etc., variables we should expect that the factor pattern would yield (1) a general factor, corresponding to the total ergic endowment (11), (2) a bi-polar factor, positive in the c + d derivatives and negative in the a + b derivatives, (3) superimposed bi-polar factors subtending a smaller number of variables, actually two or three in these instances. The relation of the magnitudes of the variance due respectively to the positive general factor and the bipolar factors will depend on the relative importance of the (presumably hereditary) differences in total endowment in this erg (drive) and the (presumably environmentally-determined) individual differences in cathexis, or internal and external inhibitions.

It would seem, therefore, that with suitable criteria to permit a realistic rotation of axes, factor analysis could detect and delimit common dynamic unities. We could not, of course, measure directly the strengths at the deeper, non-overt levels of dynamic integration indicated in Fig. 1, but these could be deduced factor patterns, discovered as a plumber could deduce the volume of water running through street conduits merely from observing the faucets in use in many houses. The picture would also be complicated by the fact that the type of dynamic connection indicated only lightly in the diagram, namely a confluence of drives, through which any piece of high level behavior springs from several basic drives, would in most human behavior be far more frequent.

The mathematical picture would therefore be expected to be more complicated, involving many more factors, than that which we are at present accustomed to find in factor analysis of more constitutional kinds of traits. But the method, with suitable improvements, seems to be rightly oriented for discovering the major sentiments which form common traits.

An indication of its correctness and practicality is offered by one of the few soundly based analyses of orectic traits yet available, namely Burt's study (6) of recorded (not rated) emotional behavior in children. There emerged precisely the type of structure here argued on theoretical grounds: a general factor of total emotionality and various superposed bi-polar factors corresponding to alternative expressions. Even without elaboration of mathematical procedures the method could be successfully applied to animal motivation studies, in which the highly complicated sentiment and habit superstructure of human beings would only be faintly represented.

Dynamic traits, however, in contrast to the two remaining types of trait unity, can be investigated by other means than the factor analysis of individual differences in behavior. For there exist a number of approaches which we may call *temporal sequence methods*, or intra-individual studies.
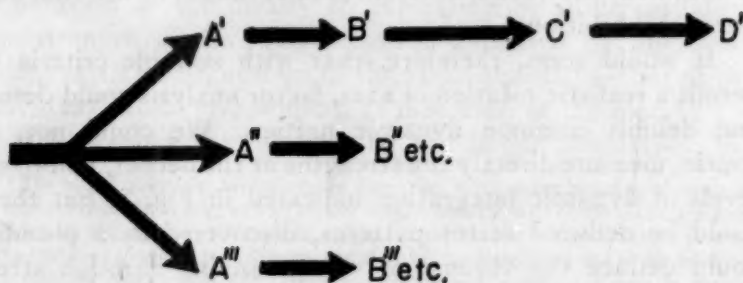
FIG. 2.

Temporal sequence study looks for covariations within the individual instead of variations between individuals, as in factor analysis. It is, in other words, an intra-individual rather than an inter-individual method, but this is only one aspect of the difference. There are really two problems and two methods here, and if we wish to use existing terms which approximately indicate the difference we have in mind we should call them the problems of equivalence and of subsidiation or purposive sequence. Figure 2 puts these problems schematically. A′, A″, and A‴ are forms of behavior

springing from the same ergic root and directed to the same biological goal. They are, in Allport's terms, equivalents. A', B', C', etc. are successive pieces of behavior on the path to the goal by way of the A' route. They are items in a purposive sequence. The investigation of the structure of a dynamic trait (in this case, of course, an individual, unique trait, though it may be common if others also possess it) requires elucidation of both kinds of connection: equivalence and purposive sequence. These correspond to bipolar and other factor structures in the factor analysis approach.

Now it is sometimes claimed that the first kind of connection—equivalence—can be established by inspection of the quality of the behavior, namely of the symbolic and other resemblances which enable the insightful psychologist to discover by intuition the equivalence of purpose, as when he explains to you that a man kicking a chair really wants to punch his opponent's head. Similarly some psychologists suppose that purposive sequence or subsidiation can be discovered without study of covariation, for in clinical practice and everyday life it is usual simply to ask a man for what more remote purpose he acquires a certain piece of behavior, or we make an intuitive judgment or we ask him to introspect by way of free association.

It is to be noted, however, that even in the hurried conditions of routine work we do not entirely trust these short cuts. We do not believe a conversion hysteric's explanation of why he cannot walk, or our friend's rationalizations; nor can one tell from inspection of style whether a man who is thumping a piano does so to release aggressiveness, lust or fear. In fact dynamic, teleological links have to be discovered, in the end, like any others, by observing how events vary together.[8] This bleak truth is less escapable when one considers studies of animal motivation.

[8] It is contended by Allport, on the other hand, that the unity of a trait is such that from understanding it one can predict the individual's behavior in a field with regard to which there have been no previous observations made. This is a prediction from the formal, essential nature of the trait. In the view of the present writer this might be done, but only when the formal or intentional nature of the trait has been established by factor analysis. Thus if we find that all the highly 'g' saturated performances involve the eduction of complex relations we may risk the prediction

Temporal sequence studies aim at detecting these covariations and they do so essentially in two ways, corresponding to the two kinds of problem. First we may observe temporal variations in two or more pieces of behavior and see if a sequence of observations on one correlates with a sequence of observations on the other. Thus if we plot an individual's sociability and his interest in the opposite sex and find the daily variations are negatively correlated we may conclude that these two kinds of behavior are equivalents, like A' and A'' in Fig. 2. An all or nothing variation, as in the observation of the repeated disappearance of one symptom with the coincident emergence of another, constitutes a special case of such correlation. This *method of temporal covariation* permits experiment, in addition to passive observation, for one can, for example, remove one form of behavior, by deprivation or inhibition, and record changes in the other.

To interpret the structure and general nature of the dynamic connection in such a case, from the magnitude of the correlation, is, however, no simple undertaking. For example, a complete absence of significant correlation in temporal covariation studies does not prove absence of 'equivalence' in the two pieces of behavior concerned. Decline in A' behavior may result in an increase of A''' behavior rather than a change in the particular variable, A'', one has chosen to pair with the first. In fact we meet again here the problem we have already encountered in the cross-sectional, factor analysis approach, but here its manifestations lie in the variations in a single individual. For here too there will be a general factor among the increments, due to the tendency for equivalent elements of a purposive behavior trait to vary together as well as inversely. The common variation will arise here, not from differences in hereditary endowment but, for example, from endocrine changes or swings of appetite,

that a person with the trait of high intelligence will do well in a situation obviously characterized by the need for complex relation eduction, even though we have no previous experience of the correlation of this test situation with the tests by which the trait 'g' was identified. However, in so far as our conceptualization of the essence of the trait might be faulty or limited, such prediction would always involve some guess-work.

producing covariation in all manifestations of, for example, sexual behavior. On the other hand the bipolar factors will result, as in the previous case, from circumstances of internal and external inhibition, and will produce inverse common variation of equivalent (*i.e.*, alternative) manifestations of the disposition. It seems desirable, therefore, to pursue longitudinal, temporal covariation studies by observing variation in quite a number of manifestations at the same time, rather than in a pair only.

The second form of temporal sequence study, which deals with the unravelling of purposive sequence or subsidiation (**22, 11, 23**), we may call the *method of temporal invariance*. It is concerned with finding out what invariably follows what. It is therefore identical with the search for any kind of causal connection, except that in teleological, final causation one is interested in the sequence in both directions.[9]

The aim of temporal invariance study is to find out for what more remote purpose a given piece of behavior is being carried out, *i.e.*, to discover the A′ to B′ to C′ linkages in Fig. 2. One piece of behavior (a trait element) serves and is connected with another if the two form an invariable sequence. The more remote goal is the behavior or satisfaction, among many following pieces, which alone invariably follows. Psychoanalytic free association is a mathematically unchecked, introspective and loose application of this method. As in the preceding method, deliberate experiment, by manipulating dynamic successions and by stimulating and depriving, can supplement passive observation. Generally this particular longitudinal approach requires little or no mathematical elaboration, the task being one only of recording frequencies of behavior elements preceding or following a given piece of behavior.

Dynamic unities, we have argued, are of two kinds: ergic unities, *e.g.*, needs and dispositions, in which all behavior

[9] In efficient causality one looks for the invariable predecessor; in final, teleological 'causality' for the invariable successor. The present writer considers that all teleological causality in psychology is ultimately a manifestation of efficient causality, but that it may be advantageous to establish the latter by first establishing the former, which is simpler.

elements directed by intention to one biological goal are abstracted (this appears as a general factor in factor analysis); and metanergic unities, *e.g.*, sentiments, in which drives from different ergic roots fuse in a cathexis upon a single real object. Temporal sequence study, in either of the above sub-methods, seems to be the *only* means, in extra-individual study, by which either type of unity can be discovered. The method suggested by Baldwin (5) to explore the sentiments of a single individual by recording the frequency of coincidence of diverse emotional reactions to the same objects, is a variety of sequence study, since the identification of the drives involved in the sentiment fusions depends on sequence study.

Among other suggested alternatives to the basic methods here described as exclusive is the method of validating a trait entity by 'prediction.' One writer argues that "successful prediction from life history would establish traits and nothing in this process demands factor analysis, covariance or even quantity." But unless the prediction proceeds successfully from one observed element in the trait to *an entirely different element* (in which case the proof rests on exactly the same grounds as correlation through simultaneous covariation, above), the prediction provides evidence of nothing more than consistency, reliability or absence of function fluctuation in the trait element concerned. Function fluctuation is, of course, an issue distinct from, and simpler than, that now being discussed.

In exploring dynamic trait unities, then, we are confined definitely to the study of covariance, in inter-individual and intra-individual circumstances. Only the latter circumstance is capable of leading to knowledge of truly unique traits. The collation of both kinds of results, however, permitting sequence study to aid the choice of factors in factor analysis data, seems necessary at present for the successful exploration of common traits. For unless unforeseen methodological improvements make factor analysis more self-sufficient and definitive, and allow us to handle the problem of unknown and mixed levels of dynamic trait manifestation, the sequence study must remain not just an auxiliary but a necessary preliminary.

V. Basic Methods in Determining Trait Unities: The Situation for Traits in General

Traits other than dynamic traits cannot be investigated by sequence studies, for constitutional and social mould traits do not involve, in the trait unity, striving for a goal. The covariation of elements which constitutes the unity of these traits has to be discovered by that inspection of correlation coefficients which has been called factor and cluster analysis.

The straightforward application of factor analysis, however, may be insufficient to bring out these trait unities. For it is obvious, in the first place, that the factors, clusters or correlation patterns arrived at depend, in their nature and number, upon the sources of variability in the particular population used. In most factor analysis it has been customary to make the population homogeneous for those aspects of personality with which one is not particularly concerned, *e.g.*, age, sex, education, natio-racial sample, cultural background, etc., in which case one considers the pattern established only for persons with that background. For the shape of clusters, the factor saturations of variables and even the very emergence of a factor will depend on this preliminary arrangement. The point is illustrated by such a common observation as that stature or reading speed will correlate highly with the general ability factor in an age-extended child population, but little or not at all in an adult group.

Now it is argued here that a deliberate manipulation of the population sample and its circumstances, in order to contrast the results of different circumstances, may be necessary to discover trait unities of various kinds, and that factor analysis has to some extent failed in clarifying the field of personality because it has confined itself to analysis of variations as they exist in a typical mixed population at a given moment. This statement applies to Q-technique as well as R-technique, for though the former has certain advantages in exploring personality aspects otherwise difficult to approach, it yields, as Burt has shown (7), the same factors as R-technique on similar populations.

An important new source of variations for exploring trait unities lies in the *increments* obtained in a population with age, with training or cultural influences with physiological influences and with experiences affecting personality. If one employs this method as a check on the ordinary factor analysis, the emergence of the same factors would indicate that one is dealing with real functional entities instead of mere mathematical conveniences. Thus if, for example, the 'c' factor of surgency (extraversion core) is found by static factor analysis to saturate especially such traits as sociability, quickness of apprehension, originality, informality and cheerfulness, we should expect the same factor to emerge and saturate, proportionately, the same traits in an analysis of the increments of these traits occurring in a given group of individuals with increasing age. That is to say, not only should the more sociable individual be more quick, but, if surgency is to be considered a real, psychological, functional entity, we should find that the individual grows more quick as he grows more sociable.

Other studies which do not depend on existing individual differences in a group would be found in measurements of *fluctuation* of traits (not steady increments with age or other influences but short term variations), for we should expect the elements of a true functional unity also to fluctuate together.[10] Again analyses could be made of the correlations of the differences of twins reared apart, a procedure which might be expected to eliminate the correlation clusters due to constitutional traits leaving only those due to dynamic and social mould traits. Incremental, fluctuation and hereditary relation factor analyses might be grouped apart, under the general label of *differential factor analyses* to distinguish them from those *static factor analyses*, on homogeneous or non-homogeneous populations, in which the existing differences in a group of individuals at a given moment are taken as the basis of correlation. Our contention is that personality study requires the extension of the present static factor analysis

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[10] This is the method, though without factor analysis, already implied by, for example, the study of cyclothyme traits by Johnson (17).

researches by the inclusion of differential factor analyses, and that the only factors which can be regarded as corresponding to real trait unities are those which emerge from both approaches.

Even with this principle in mind, however, the research worker has still the task of extracting, from any given set of correlations, clusters and factors, the psychologically meaningful unitary traits which may lie behind them. How this task may be accomplished is best seen by imagining unitary traits of the types set out above and deducing the structure of variables which would follow from them. By this approach from within we may see how the researcher will need to proceed in the reverse process of trying to arrive at the traits from the correlations.

In Fig. 3 the eight lines beneath the letters represent a set of trait elements—fragments from the total surface of personality—chosen for rating and inter-correlation in a typical sample of the population. Let us suppose that there were

SOCIAL AND PHYSICAL PRESSURES
AND DEMANDS
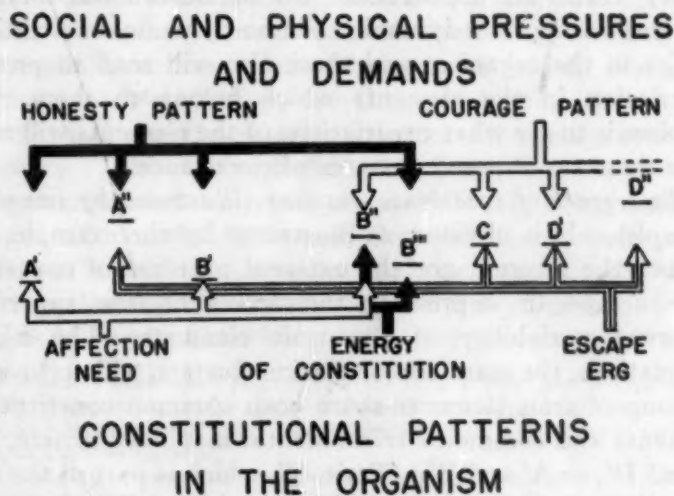
FIG. 3.   Definition of trait elements.

A'.   Respecting property of friends.
A". Respecting property of strangers.
B'.   Being truthful to friends.
B". Being truthful to strangers.

B'''. Avoiding exaggeration in relating stories.
C.   Facing physical dangers.
D'.   Keeping one's head in emergencies.
D". Magnitude of vegetative n. s. response in fear.

originally, in the mind of the investigators, four traits—
A, B, C and D—each constituting some logical, traditional,
stylistic or semantic unity, and that they were broken down
into narrower, behaviorally-defined traits—A', A'', B', B'',
B''', etc.—as a precautionary step, to avoid the trap of false
presuppositions from verbal unities.

Trait elements which vary together, in magnitude of and
direction of variance, will be drawn, by convention, at the
same level on this diagram. Now the forces which act upon
these behavior elements and which cause them to vary and
covary are of two kinds. On the one hand there are patterns
of external stimulation and inhibition, corresponding to the
social and environmental mould or die as already described.
By reason of the slight or deep impress of any particular die,
i.e., of any group of environmental forces which, for social or
other reasons happen to operate together, from a single focus,
the individual will have a high or low development of the set
of trait elements constituting that pattern, *i.e.*, they will
covary within the population. On the other hand there are
patterns arising from dynamic and non-dynamic constitutional
unities in the organism and these also will tend to produce
covariation in the elements which belong to them. The
problem is to see what covariations of the elements will result
from these super-posed patterns of covariance.

*Emergence of covariance patterns (illustrated by imaginary
example).*—It is obvious, as illustrated by the example, that
neither the internal nor the external patterns of covariance
will succeed in impressing themselves on the superficial,
observed variability of the trait elements. The highest
correlations, the manifest correlation clusters, will occur where
a group of trait elements share both common constitutional
elements and common environmental mould influences, as in
C and D', or A' and B'. Trait A'', which is part of the same
honesty trait and trait B'' which is part of the same dynamic
need (with regard to A' and B', respectively) do not show
more than moderate correlations with other items in the same
trait. The most negligible correlations are those among
elements in logical, stylistic, semantic unities (*e.g.*, B' with
B'').

Our conclusion therefore is that *factors*, which represent sources of covariance, rather than high *clusters*, correspond to traits in the above senses. This conclusion, incidentally, does not deny the fact that there are great difficulties in the way of choosing, from a great variety of factor sets arrived at mathematically, the one set which alone represents the psychological reality. That one should follow this difficult and uncertain path of discovering factors, instead of accepting the view that an empirically obvious correlation cluster [11] constitutes a trait, requires some justification. The justification is (1) That meaningful traits, in the sense of dynamic, constitutional and social mould unities, correspond to factors rather than clusters. (2) That factors may be expected to recurr, in identifiable form, in a variety of populations, circumstances and modes of factor analysis (*e.g.*, in differential as well as static correlation analyses). A constitutional hyper-thyroid pattern, for example, may appear identifiably in different cultural groups. A cluster, on the other hand, may be a local artefact, corresponding to a chance overlap of social mould and constitutional trait factors. (3) That the factor traits have the value of being more widely useful in calculation and prediction, for they correspond to the influences that are functionally distinct in the growth of personality. This is true even of social mould traits, which have no functional unity in the individual, being only a collection of correlating habits, but which represent the influence of a single force in the environment. Even if this force may never operate again,[12] in that the connection of the elements is purely historical, the factor constitutes a con-

[11] It should be made clear, perhaps, that the term 'cluster' here is used in its simple, obvious sense, not in the special technical sense employed by Tryon in his technique of 'cluster analysis' (31). The latter seems to be a novel way of arriving at factors of a general character.

Moreover, it is necessary to remember that *some* clusters will be due to all the component traits' having a high saturation with one factor. To say that clusters are factor overlaps assumes that other things (factor saturations) are approximately equal.

[12] This observation reminds one that the social mould patterns may be expected to be far more complicated than the constitutional ones. This occurs not only because of the great variety of social mould influences and their changes with place and time, but also because, as the individual grows older, they operate differently or vanish entirely. His nature is, in respect to these patterns, a geological deposit.

venient measuring rod, for it has once operated with a similar pattern on all individuals, producing similar features.

The above discussion illustrates a certain danger in the term 'operational unity,' which has sometimes been applied to simple, empirical clusters of correlation coefficients, on the grounds that the trait elements involved certainly do, operationally, 'go together.' Strictly, this term should be applied just as much to factors. For what shall be called an operational unity depends on the nature of the operations one has in mind, and if they are widely conceived, to include all varieties of psychological circumstance and situation, the factor is the truer [13] operational unity.

To avoid undue complication of presentation the problem was not raised, in relation to the diagram above, that the ergic or constitutional roots of a given social mould trait pattern might be different for different people. The mathematical analysis takes care of this, presenting a composite, average picture. It seems extremely likely (indeed certain if one adopts a holistic psychology) that each constitutional trait shows itself to some extent in every piece of behavior. That is to say it will appear as a general factor. Its saturation of any trait element will thus be a function of the average extent to which, in the population, the particular ergic root or constitutional tendency enters into the formation of the trait.

Finally we have to note that all the above methodology says nothing about the permanence of traits within the individual. Except in factor analysis one is taking a picture with a flashlight, discovering configurations that exist at a given moment. (In most rating, as opposed to test, studies, it would be a fairly long 'moment.') Thus one might catch, in addition to what are usually called traits, the patterns of states of maladjustment or even moods, needs-in-action, and physiological transient states. These patterns could be sorted out from the more permanent ones by examining consistency coefficients over various intervals.

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[13] This does not deny that some clusters may correspond to traits. The highest observable cluster may, even so, be an overlap of two such 'real' clusters.

What degree of permanence is required to constitute a trait rather than a state is a relatively arbitrary issue.

VI. The Utilities of Traits, Attitudes, Sentiments and Interests, as Concepts

The present discussion on methodology cannot be seen in the general perspective of personality research as a whole without a brief clarification of the relation of traits to sentiments, attitudes and interests. The present writer has pointed out elsewhere (8) the confusion and loss brought about by incoordinated terminology in this field, and has suggested that the trend of discourse has been to crystallize 'attitude' to mean a finer ramification of a sentiment (related to it as a twig to a bough), so that a collection of attitudes grow out of a sentiment, in its interactions with environment. The sentiment, being deeper, expresses itself more often in feelings and actions; the attitude in opinions. This relationship, of course, dismisses the use of attitude to describe a merely momentary mental stance, state or experience of derived emotion, and retains the term strictly in the sense of a neuro-psychic disposition, i.e., a mental structure, or, if structural connotations are not desired, in the sense of a potential and recurrent pattern of behavior.

It has, further, been suggested in the above systematization that 'interest' best applies to the quantitative—as contrasted to qualitative or directional—vector aspect of metanergs (attitudes, sentiments) and ergs (primary drives). An interest, however, may loosely be used to define also, in terms of its goal object (direction or quality), any erg or metanerg of which it is the quantitative aspect. Thus we properly speak of amount of patriotic interest, but we loosely speak of patriotic interests instead of sentiments.

What, then, is the relation of the term trait to these terms? That depends upon the trait. Only dynamic and social mould traits have any relation to sentiments, attitudes and interest. Indeed, the second, metanergic form of dynamic trait is nothing but a sentiment, being an organization of drives about a real object. Social mould traits, on the

other hand, are collections, often dynamically unintegrated, of sentiments, attitudes and habits.

Actual language usage, as already indicated, gives heavier emphasis to the use of 'trait' with respect to constitutional and ergic (dispositional type) dynamic traits. Such usage implies that sentiments (metanergic dynamic traits) are more specific and local than traits, as attitudes are more specific than sentiments. The justification for this habit of language lies in such observations as that of a recent research (2) to the effect that individuals can undergo a complete cultural transplantation, changing attitude and even sentiment attachments, without manifesting any appreciable transformation of what is generally spoken of as personality (traits). But, as we have seen, all traits, even common traits, slowly change.

Great as the need is for some general term to refer to species of personality manifestations, the practice of restricting 'trait' to the deeper, more important, stable and abstracted variables of personality has some justification, in usage and meaning. For normally we infer a sentiment from a collection of observed attitudes and we similarly abstract an individual's trait rating from observations of the character of his sentiments. Thus, for example, we judge that a man has an assertive disposition because we are presented with an accumulation of sentiments and attitudes loaded with assertiveness. He may be very assertive in some and submissive in others. We strike an average.

The abstraction which we perform in rating a trait is also indicated by the fact that the object of an attitude, and to a lesser extent of a sentiment or complex, can always be designated, whereas the trait is an attitude to life generally. (Where ergic dynamic traits are concerned it is the attitude of stressing a particular biological goal.) This generalization of the trait, in contrast to attitude, is only another way of expressing the mathematical statement that it is a 'factor' derived from a collection of particular responses intercorrelating; or of saying that this conception of trait is of common traits, whereas sentiments and attitudes are more frequently unique 'traits.'

Nevertheless, it is not possible simply to equate the antithesis 'common-vs.-unique' with 'constitutional and ergic-vs.-metanergic,' or with 'important, major trait-vs.-unimportant, superficial trait.' For a very narrow, superficial attitude, and one not even remotely related to constitution, may yet be a thoroughly common trait. Again, a constitutional trait (as produced by biological variation or mutation) may be highly specific and unique. Similarly a unique trait may be highly important, at least in relation to the individual personality. In short, we have to admit three or four[14] distinct characteristics of traits, even though they correlate so that in general the more constitutional is also the more common and the more important. In conclusion, therefore, this semantic problem seems most reasonably solved by applying the term trait generically to all manifestations of personality. Where the dictionary or a factor analysis indicates 'common traits' we are dealing with the more substantial manifestations, whereas unique traits are more likely to be found among such slighter manifestations as attitudes and some sentiments.

The general question of the relationships between various types of trait unity cannot be left without some brief, and necessarily rather speculative discussion concerning the issue of the relative 'importance' of common-vs.-unique traits and of the three varieties of common traits. A language count would, of course, vote that common traits are far more important than unique traits, and the views of many psychologists imply the same judgment. It is probably true that the behavior of most individuals in a mass-educating culture can be fairly completely predicted in terms of common traits; but under more special circumstances the unique trait can be the major factor in personality prediction. Again the dictionary gives, among common traits, a seeming predominance of dynamic terms, supporting the clinic in its emphasis on the first of the three varieties of unity.

[14] Another characteristic difference, implied by the above discussions, is that common traits can be measured in units derived from the dispersion of a population (which, for lack of a better term, we will call 'normative' units) whereas unique traits, having to be defined logically, can only be measured in 'metric' units. Common traits can be measured in both metric and normative units.

The relative importance and utility of the three varieties of common traits could theoretically be expressed precisely in terms of the proportion of the total variance (in a number of representative fields of behavior) contributed by traits of each type. From a glance at available factor analyses one might hazard the guess that constitutional traits, *e.g.*, surgency, intelligence, general emotionality, take an appreciable slice of the variance. Further, there is some clear evidence, *e.g.*, in the lower 'g' saturation of intelligence tests with adults, and in the higher inter-correlations of forms of honest behavior in older and mentally older subjects (**15**), that social mould traits become more definite and account for more of the variance as individuals become longer exposed to the culture. Certain social mould traits, notably the 'w,' character integration factor, obviously contribute a major amount to the variance in very important fields; but in general one may suspect that the utility of social mould traits is reduced by the fact that they are specific to a culture—to some extent even to the provinces of a culture—so that their predictive value is apt to be local or temporary.

VII. Résumé

1. All traits are really unique, but in a population with common racial and cultural backgrounds a majority are so nearly common that they can be treated as common traits, measurable on common axes.

2. It is contended, with Gordon Allport (**1**), that "it is more important to discover intelligible traits than independent ones," *i.e.*, mathematically independent ones, for the former have functional existence in the personality and society, and can be more widely used in prediction. Such intelligible unities seem to be of three kinds—*dynamic, constitutional* and *social mould*. *Co-nascent* and *logical* trait unities also have utility in special circumstances, but their present too facile and frequent use in education and guidance seems mistaken.

3. These three kinds of traits manifest themselves as mathematical factors (not necessarily, or even probably, of an independent kind) in the factor analysis of trait element

inter-correlations. To discover them, however, it will be necessary to collate a variety of *static factor analyses* with a well chosen variety of *differential factor analyses*, thereby evolving criteria for the rotation of axes distinct from the unpsychological methods—such as 'simple structure'—now employed.

4. Dynamic traits alone may be supplementarily investigated, both as unique and as common traits, by *temporal sequence studies* which are longitudinal, intra-individual methods.

5. Clusters (of highly positively inter-correlating trait elements) are unlikely to be traits. Dynamic traits, one may deduce, are likely to manifest themselves as general factors with superimposed bi-polar factors. Constitutional traits will appear as simple general factors, probably with a more even saturation of behavior elements than is found for dynamic general factors. Social mould traits are likely to appear as much restricted group factors. Such considerations contribute towards, but do not provide, a unique determination of trait unities by factor analysis. The possibility of a truly unique solution to a factor analysis, yielding the psychologically real trait unities in personality, is discussed in a later article (**10**).

6. All traits, being relations between a changing organism and a changing environment, are only temporary patterns. The common traits, however, are likely to be at least as stable as a culture pattern.

7. Common traits can be measured in either metric or normative (population relative) units; but unique traits, having to be defined by logical dimensions, can only be expressed in metric units.

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A QUANTITATIVE THEORY OF DECISION

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I. INTRODUCTION

The mark of maturity of a science is the extent to which it can state its laws and functional relationships in precise mathematical terminology. Statistical techniques, especially those employing the theories of probability, have found wide application in psychology. Some geometrical concepts, particularly those of topology, have also been use dalthough somewhat less widely. Rarely, if ever, however, has there been a thorough application of both types of mathematical means in psychological theory.

Recently in psychophysics an attempt has been made by Cartwright (5) to utilize geometrical concepts in the construction of a theory which brings psychophysics and 'motivation psychology' closer together. We shall here attempt to bring statistical and geometrical concepts together in a quantitative theory of decision.

Cartwright presented a theory of decision-time in topological and vectorial terms which leads to the gross prediction that decision time approaches a maximum as the relative frequency of a given response approaches fifty per cent. This theory makes congruent otherwise unrelated and apparently inconsistent psychophysical data. In the present paper we shall make a refinement and further quantification of the theory so that the absolute values of *both* decision-time and relative frequency may be predicted for specified conditions.

The discussion of these refinements will be facilitated by

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a summary of the earlier statement of the theory.   Cartwright assumed that the forces determining judgment time in a psychophyical experiment operate in essentially the same way as those determining any sort of decision. A topological representation of a simple decision situation and the forces which determine its outcome are reproduced in Fig. 1.   In this
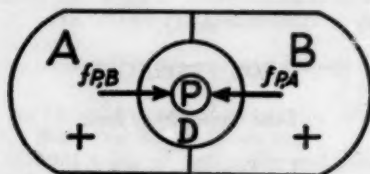
FIG. 1.   A topological representation of a decision involving two alternatives. Region P represents the person, region D represents the activity of deciding, and A and B stand for the two alternatives.

hypothetical case only two alternatives (A and B) are possible, and, assuming that they are both attractive, two forces ($f_{P,A}$ and $f_{P,B}$) act upon the person in the direction of these alternatives.

From the definition of the concept of force it follows that the person will choose the alternative toward which the stronger force is directed, and moreover, that *he can choose neither alternative as long as the two forces are of equal magnitude*. Assuming that time is required for an initial balance of forces to become upset, we can conclude that whenever a balance of forces arises, decision-time will be lengthened.

When a subject is asked to make a judgment about the nature of a stimulus, the goal of a cooperative subject is "to be right."   If the stimulus is clearly longer, the region, "saying 'longer,'" L, is contained in the region, "being right," R, since "saying 'longer'" and "being right" are viewed as equivalent.   Since the region "being right" has a positive valence, and since L is included in R, the force correlated to the valence has the direction toward the region L ($f_{P,L}$).   Similarly, the region "being wrong," W, has a negative valence and contains the region "saying 'shorter,'" S. The force related to the negative valence will, therefore, be

directed away from S ($f_{P,-S}$).   Since, the forces $f_{P,L}$ and $f_{P,-S}$ are equivalent in direction, the sum of these may be considered as coordinated to the positive valence. The force acting upon P is clearly in the direction of the region L, and the subject will, therefore, respond "longer" without hesitation.

Similarly, when the variable stimulus is clearly perceived as shorter than the standard, the force on P is directed toward S and the subject will respond "shorter" without hesitation.

When the stimulus falls upon the boundary between the regions in the phenomenal field it is equally probable that either judgment will be correct.   The region "saying 'longer,'" L, is thus contained in two overlapping regions: "being right," R, and "being wrong," W. The region S, also is contained in the two regions R and W.   In other words, both situations discussed above are functioning simultaneously.   The presence of these two overlapping situations places the person between conflicting forces.

The strength of each of the forces acting on the person will depend upon how much weight or potency each situation has for the subject. Stated formalistically, in overlapping situations, the strength of the effective force acting on a person ($^{o}f_{P,G}$) is equal to the strength which that force would have if there were only one situation, *multiplied by* the potency of the situation to which it belongs (**18**):

$$^{o}f_{P,G} = f_{P,G} \cdot Po(S). \tag{1}$$

The potency of each overlapping situation is determined by the subject's feeling of probability that his judgment is correct.   For example, if the subject feels both situations to be equally probable, the potency of each situation is equal to 0.5.   (The sum of the potencies is arbitrarily set equal to 1.)   The person is located, therefore, between opposite forces of equal magnitude and, as in Fig. 1, cannot change his psychological position (*i.e.*, make a judgment) until the situation changes.   Time will be consumed before this change can occur and we may, therefore, expect judgment-time to be lengthened when the stimulus falls on a boundary in the phenomenal field. Assuming further that the balance of

forces will be upset in each direction equally often, repeated presentations of this situation will not only give a lengthened average judgment-time but also a relative frequency of judgment of longer (or shorter) of 50 per cent.

In another article, Cartwright (4) presented data which strongly support the theory by showing that the decision time curve reaches a peak when the stimulus approaches the boundary between two categories of response. The average distance between the 50 per cent point of relative frequency and maximum decision time was 0.6 units of the stimulus scale.

II. MATHEMATICAL STATEMENT OF THE THEORY OF DECISION

Aside from its inability to lead to more than gross quantitative predictions, the theory presented above is deficient in one other important respect. As it stands, any imbalance of forces, no matter how slight, is said to lead to a decision. Actually, a person usually will not announce his judgment before a given magnitude of difference between driving forces is reached. In any situation which can be set up in an experiment, restraining forces are also present which prevent the subject from going off 'half-cocked.' The person wishes to be reasonably certain that he is correct. In other words, the resultant of the driving forces ($f_{P,L}$ and $f_{P,s}$) must have a greater magnitude than the restraining force opposite to leaving D, ($f_{\overline{D,-D}}$). Only when this state of affairs is reached can a decision occur. In short, decision time may be lengthened even when the conflicting driving forces are not exactly equal.

A quantification may now be accomplished by making certain assumptions about the way in which the driving forces and the restraining forces fluctuate in time. Here, as in any other case, one can observe only the effect of the resultant force not its single components.

It is a common procedure to start with certain assumptions concerning the single forces. From them certain statements concerning the nature of the resultant force can be derived synthetically. Mathematically, this constitutes a

procedure of defining the original distributions of forces and of deriving the resultant distribution. The conclusions may then be tested empirically. We will proceed in this order.

1. *Fluctuation of potency.*—For any given physical setting in which a decision or judgment is to be made, we assume that the potency of each situation fluctuates in time and distributes itself normally about a given mean value, with variance equal to σ_{Po}^2. More precisely, the distribution of potency in time, for an infinite length of time may be defined by the distribution function:

$$f(Po) = \frac{1}{\sigma_{Po}\sqrt{2\pi}} e^{-\frac{(Po - M_{Po})^2}{2\sigma_{Po}^2}}. \tag{2}$$

2. *Fluctuation of force.*—As seen above, fluctuations of potency cause the forces toward making a decision to fluctuate according to the potency fluctuation. In a setting where a judgment of longer or shorter must be made, the force toward saying longer ($f_{P,L}$) will fluctuate normally in time about a given mean value and the force toward saying shorter ($f_{P,S}$) will also fluctuate about its mean value. The equations of these two distribution functions are written in exactly the same form as that for potency except for the substitution of the correct values of the mean and variance which characterize these distributions.

The relative potency of one situation is, by definition, one minus the potency of the other situation. Therefore, the variance of the potency distribution functions for both situations are equal. Accordingly, since the fluctuation in force is due to this fluctuation in potency, the variance of the distribution of forces toward "saying 'longer'" will be equal to the variance of the distribution of forces toward "saying 'shorter'" ($\sigma_{f_{P,L}} = \sigma_{f_{P,S}}$).

3. *Fluctuation of differences between forces.*—At any time t there occurs a pairing of randomly selected forces,[1] one

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[1] The following questions arise concerning the nature of these fluctuations. If at t_1 a given potency exists, then at the immediately following t_2 can any other potency, within the range of fluctuation, exist, or must the fluctuations be continuous? It may be seen, however, that the choice between an assumption of continuous or discontinuous fluctuation does not affect our analysis or the derivations. In either case the distribution functions and the probabilities of obtaining given pairings are the same, and in either case, the initial potency at the time of presentation of the stimuli is a matter of random occurrence.

toward "saying 'longer'" and one toward "saying 'shorter.'" Let us call the distribution of the differences between these opposite forces the z function. This function, for an infinite number of pairings can be defined from the two force distributions. The z function distributes itself normally about its mean, which *is equal to* the mean of the distribution of forces toward "saying 'longer'" *minus* the mean of the distribution of forces toward "saying 'shorter.'" (Positive is the direction of longer and negative the direction of shorter.) The variance of the z distribution is equal to the variance of the $f_{P,L}$ distribution plus the variance of the $f_{P,S}$ distribution, plus two times the product of the two sigmas ($\sigma_z^2 = \sigma_{f_{P,L}}^2 + \sigma_{f_{P,S}}^2 + 2\sigma_{f_{P,L}}\sigma_{f_{P,S}}$). The last term is added because the pairings from the two force distributions will have a perfect negative correlation, since their respective potencies are perfectly negatively correlated. Since $\sigma_{f_{P,L}} = \sigma_{f_{P,S}}$ the variance of the z distribution will be four times as great as the variance of either of the force distributions. This z distribution is the distribution of the resultant force toward making a decision. It may be written as follows:

$$f(z) = \frac{1}{2\sigma_{f_{P,L}}\sqrt{2\pi}} e^{-\frac{[z-(M_{f_{P,L}}-M_{f_{P,S}})]^2}{8\sigma_{f_{P,L}}^2}}. \quad (3)$$

4. *Fluctuation of restraining force.*—We now make specific our assumption regarding the distribution of the restraining force ($rf_{\overline{D,-D}}$) which has the direction opposite to leaving the region of decision. This restraining force distributes itself normally about a constant mean value with given sigma. It may be written as follows:[2]

$$f(rf) = \frac{1}{\sigma_{rf}\sqrt{2\pi}} e^{-\frac{(rf-M_{rf})^2}{2\sigma_{rf}^2}}. \quad (4)$$

[2] This formula is probably not fully correct. The mean of the distribution of restraining forces should also be expressed as a decreasing function of time since there is good reason to suppose that the longer the individual stays in the decision region, the weaker are the restraining forces against leaving it. We are, however, not prepared at present to make any more specific assumption as to the exact nature of this function. As long as we deal with decisions of relatively short duration, the error involved because of this omission is probably small.

5. *Fluctuation of resultant force.*—Let us now consider the distribution function resulting from random pairings of z values with rf values. We shall call this the z' distribution. It is the distribution of the final resultant forces away from the area of decision.

Obviously the pairing which occurs here is a pairing of values of rf with absolute values of z. Any pairing in which the absolute z is greater than the rf will yield a z' value which is greater than zero. Treated in this way a final resultant force greater than zero towards "saying 'longer'" is confused with a final resultant force greater than zero towards "saying 'shorter.'" There is, however, another method which keeps these two separate and which is mathematically equivalent to it. In this method the algebraic signs of the z values are maintained in the pairings. (In our z distribution we have defined positive as being the direction of "saying 'longer'" and negative the direction of "saying 'shorter.'") The function obtained by this method of pairing can be defined in terms of the already defined z distribution and the rf distribution. The z' function will distribute itself normally about its mean, which is equal to the mean of z minus the mean of rf. The variance of the z' distribution is equal to the sum of the variances of the z and rf distributions. We may write the equation as follows:

$$f(z') = \frac{1}{\sqrt{4\sigma_{f_{P,L}}^2 + \sigma_{rf}^2}\ \sqrt{2\pi}}\ e^{-\frac{[z'-(M_z-M_{rf})]^2}{2(4\sigma_{f_{P,L}}^2 + \sigma_{rf}^2)}}. \tag{5}$$

The probability that a value picked at random from the z' distribution will be greater than zero in the direction of saying longer is the integral of the z' distribution from zero to infinity, since this area includes all such cases:

$$\text{Prob. of Longer} = \int_0^\infty f(z')dz'. \tag{6}$$

The z' function is a distribution of $(f_{P,L} - f_{P,S} - rf)$. Now let us define a similar z'' function which equals $(f_{P,S} - f_{P,L} - rf)$. As in the z' distribution the probability that a value picked at random from the z'' distribution will be greater

than zero in the direction of saying shorter is the integral of the z'' function from zero to infinity:

$$\int_0^\infty f(z'')dz''.$$

This area includes all those cases where the final resultant force would be in the direction of saying shorter.

Since the variance of z' and z'' are equal ($\sigma_{z'}{}^2 = \sigma_{z''}{}^2$),

$$\int_0^\infty f(z'')dz'' = \int_{-\infty}^{-2rf} f(z')dz'. \tag{7}$$

We may therefore go ahead using the z' distribution only.

With the aid of the preceding formulations, the theoretical values of relative frequency of judgment and decision-time may now be defined.

The expected frequency with which judgments of longer will occur is equal to the integral of the z' distribution from zero to infinity (6). The expected frequency with which judgments of shorter will occur is equal to the integral of the z' distribution from $-2rf$ to minus infinity (7). Integral (6) plus integral (7) are the total number of decisions which will occur. The area under the z' function between $-2rf$ and o is equal to the probability of not obtaining a decision.

Theorem: The relative frequency of judgments of longer is equal to Integral (6) divided by the sum of Integrals (6) and (7). The relative frequency of judgments of shorter is equal to Integral (7) divided by the sum of Integrals (6) and (7).

As was stated above, values of z' between o and $-2rf$ will not yield any decision. The probability that a randomly selected z' value will not give a decision is, then, the integral of the z' function from $-2rf$ to o:

$$\text{Prob. (No-Decision)} = \int_{-2rf}^0 f(z')dz'. \tag{8}$$

The area defined by this integral may be called the area of no-decision. Since the subject must make a decision he must wait until a z' value occurs which is outside this area.

Obviously then this area of no-decision is related to decision time. The larger the area of no-decision, the longer on the average will the subject take before he makes his decision. We then state that decision time is linearly proportional to Integral (8). To convert this into units of seconds this integral must be multiplied by some constant k. One more constant must be added to the equation since even an "immediate" decision will not be immediate but will depend on the reaction time of the subject.

Theorem: Decision Time is equal to a constant times Integral (8) plus a base value:

$$\text{D.T.} = K \int_{-2rf}^{0} f(z')dz' + B. \qquad (9)$$

Since Integral (8) is one minus the sum of Integrals (6) and (7) this may also be written:

$$\text{D.T.} = K(\text{I} - [\text{Prob. Longer} + \text{Prob. Shorter}]) + B. \qquad (9A)$$

On the basis of the above theorems we may now proceed to plot our theoretical functions of decision time and relative frequency. The theorems show that two factors influence both relative frequency and decision time, namely, the magnitude of the restraining force and the mean of the z distribution. The latter, it will be remembered, expresses the difference between the force toward saying longer and the force toward saying shorter (the resultant of the driving forces). As the difference between the stimuli increases the mean of this z distribution will also increase. It seems most convenient, then, to plot our theoretical curves along an abscissa indicating magnitude of the mean of the z distribution. When plotted in this way, a separate function will, of course, exist for each mean restraining force.

In order to work with these functions, numerical values must be assigned to the constants in the equations. We have done this in the following way: The variance of the z' distribution has arbitrarily been assigned the value of unity. This value affords us a convenient scale according to which we may assign numerical values to our variables. Thus, the

means of the z distribution have been expressed in terms of the variance of the z' distribution, and this scale constitutes our abscissæ for plotting the functions. That is to say, a mean of the z distribution which is equal to the variance of the z' distribution has a value of 1. A mean of the z distribution twice this size has a numerical value of 2. We shall hereafter refer to this scale of mean values as the z *scale*.

From our theorem of relative frequency and of the z scale we now have a set of coordinate axes on which to plot curves of relative frequency. From the theorem of decision time (dropping out the constants since these do not affect the shape of the curves) and the z scale we have a matrix for
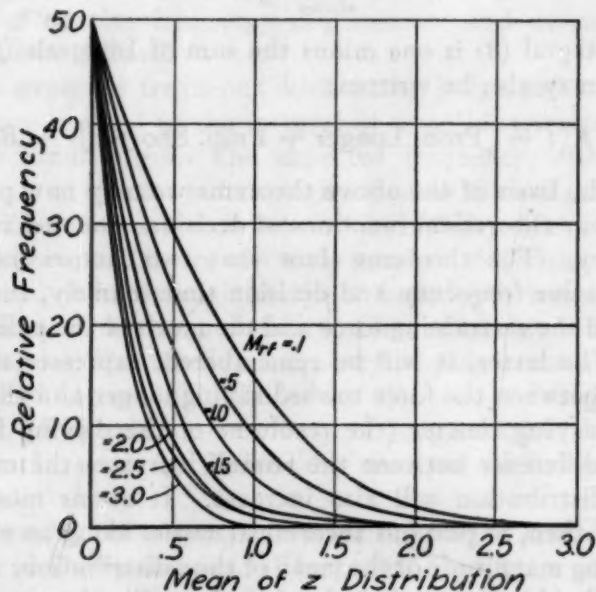
FIG. 2. Theoretical relative frequency curves for indicated mean values of restraining force ($M_{r,f}$).

plotting decision time. These curves will show us exactly how relative frequency and decision time vary with the z scale for given values of the mean restraining force. Figures 2 and 3 show families of theoretical relative frequency curves and theoretical decision-time curves respectively for the indi-
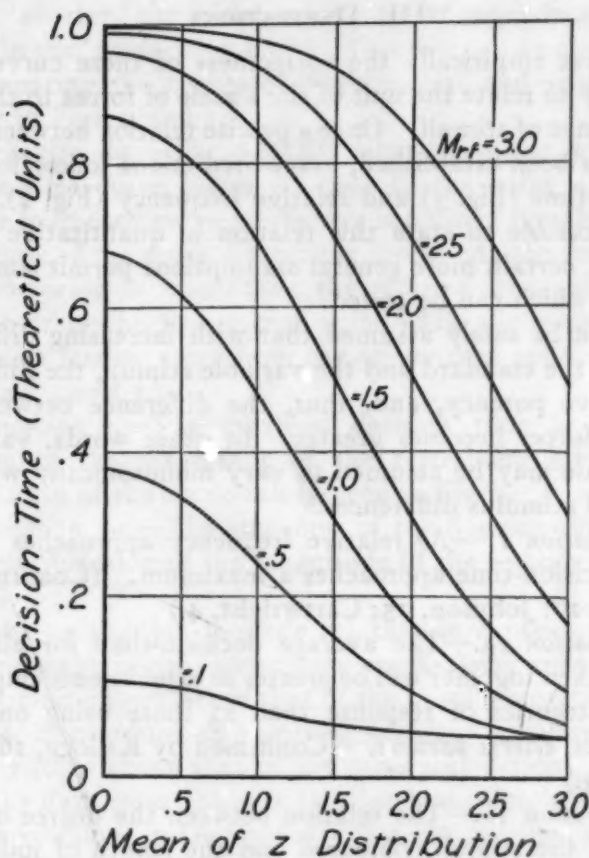
Fig. 3.   Theoretical decision-time curves for indicated mean values
of restraining force ($M_{r.f}$).

cated values of the restraining forces. (The numerical values
assigned to the mean of the restraining force distributions are
again in terms of the z scale, namely, the mean restraining
force divided by the variance of the z' distribution.) The
relative frequency curves form normal ogives. Only one-half
of each curve is plotted since the other half of the curve is
merely the plotted half rotated 180 degrees. Similarly only
one-half of each of the decision time curves is presented since
the complete curves are symmetrical about the y axis.

III. Derivations

To test empirically the correctness of these curves it is necessary to relate the unit of the z scale of forces to the unit of the range of stimuli. Once a precise relation between these units has been established, exact predictions follow for both decision-time (Fig. 3) and relative frequency (Fig. 2). If it is not possible to state this relation in quantitative terms, however, certain more general assumptions permit some predictions which can be tested.

It can be safely assumed that with increasing difference between the standard and the variable stimuli, the difference in relative potency, and, thus, the difference between the driving forces becomes greater. In other words, values of the z scale may be assumed to vary monotonically with the values of stimulus differences.[3]

Derivation 1.[4]—As relative frequency approaches 50 per cent, decision-time approaches a maximum. (Confirmed by Kellogg, 16; Johnson, 15; Cartwright, 4.)

Derivation 1a.—The average decision-time for all judgments taken together will be greater in experiments employing three categories of response than in those using only two categories, *ceteris paribus*. (Confirmed by Kellogg, 16; Cartwright, 4.)

Derivation 1b.—The relation between the degree of similarity of the stimuli compared and the length of judgment-time depends upon the location of the 50 per cent points of the curve of relative frequency. (Confirmed by Kellogg, 16; Cartwright, 4.)

Derivation 2.—The decision-time curve leaves its base farther away from the 50 per cent point than the relative frequency curves leaves 0 per cent. (Decision-time is a more sensitive measure of conflict than relative frequency.) This derivation follows from the possibility that a situation may occur where, although a difference of forces almost never arises which is large enough to give a judgment of, for ex-

[3] Essentially the same assumption is made in the theory by Cartwright (5).

[4] Those derivations which have already been stated by Cartwright (5) will be restated briefly with the references to confirmatory experimental evidence.

ample, 'shorter,' the initial pairing might result in a z' value
falling in the 'no-decision area.' This fact can be clearly seen
by comparing the curves of decision-time and relative fre-
quency in Figs. 2 and 3.

Derivation 2a.—The distance from the point where the
decision-time curve leaves its base to the point where the
relative frequency curve leaves zero is a direct function of the
magnitude of the mean restraining force. As the restraining
force increases in magnitude the relative frequency curve
rises closer to the 50 per cent point while the decision-time
curve rises farther away from the 50 per cent point. (Com-
pare Figs. 2 and 3.)

Derivation 3.—The maximal decision time is a direct
function of the magnitude of the restraining force. The
precise form of the curve can be seen in Fig. 4.

Derivation 3a.—The steepness of the decision-time curve
is a direct function of the magnitude of the restraining force
(Fig. 3).

If the assumption is made (1) that in a psychophysical
judgment experiment instructions emphasizing speed reduce
the restraining force below that of the usual instructions, and
(2) that instructions emphasizing accuracy increase the re-
straining force above that of the usual instructions, then we
may take data from Johnson (15) for immediate verification
of the predictions in Derivation 3 that the maximal decision-
time would be greatest for the accuracy instructions, next for
the usual instructions and lowest for the speed instructions.
The data for the three subjects reported by Johnson cor-
roborate the prediction:

Subject	Maximal Time for		
	Acc.	Usual	Speed
K	8.5	6.0	1.8
V	6.0	3.5	.68
Z	2.0	1.7	.52

Casual inspection of Johnson's data shows Derivation *3a* to
be corroborated. This can also be inferred from the maximal
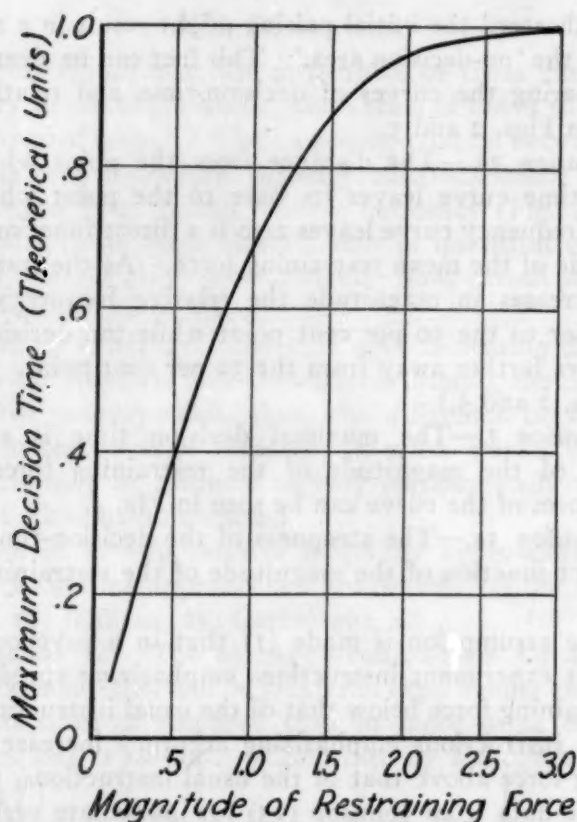
Fig. 4. Maximal decision-time as a function of mean restraining force.

decision-times given, since for all three conditions the base
values of the decision-time curve were approximately equal.

Derivation 4.—The precision of the relative frequency
ogive increases as the restraining force increases (Fig. 2).
Table I and Fig. 5 show this relationship more precisely.
Since the precision of the relative frequency ogive is used as
a measure of sensitivity in some psychophysical experiments,
sensitivity as thus measured is affected by certain attitudes
on the part of the subject.

Derivation 5.—The above derivations have assumed that
the restraining forces are equal in both directions (*e.g.*, the
restraining force against "saying 'longer'" is equal to the

TABLE I

STANDARD DEVIATION AND PRECISION OF THE RELATIVE FREQUENCY OGIVE AS A
FUNCTION OF THE MAGNITUDE OF THE MEAN RESTRAINING FORCE

M_{rf}	Standard Deviation	Precision (h)
.1	.935	.756
.2	.875	.808
.3	.820	.862
.4	.770	.918
.5	.722	.979
.6	.680	1.040
.7	.643	1.100
.8	.606	1.167
.9	.574	1.232
1.0	.545	1.297
1.1	.518	1.365
1.2	.493	1.434
1.3	.470	1.504
1.4	.450	1.571
1.5	.430	1.644
2.0	.351	2.014
2.5	.296	2.389
3.0	.255	2.773

FIG. 5. Precision of relative frequency ogive as a function of mean restraining force.

restraining force against "saying 'shorter'"). When the restraining forces are of different magnitudes, the maximal decision time, the steepness of the decision time curve and the precision of the relative frequency ogive assume values whic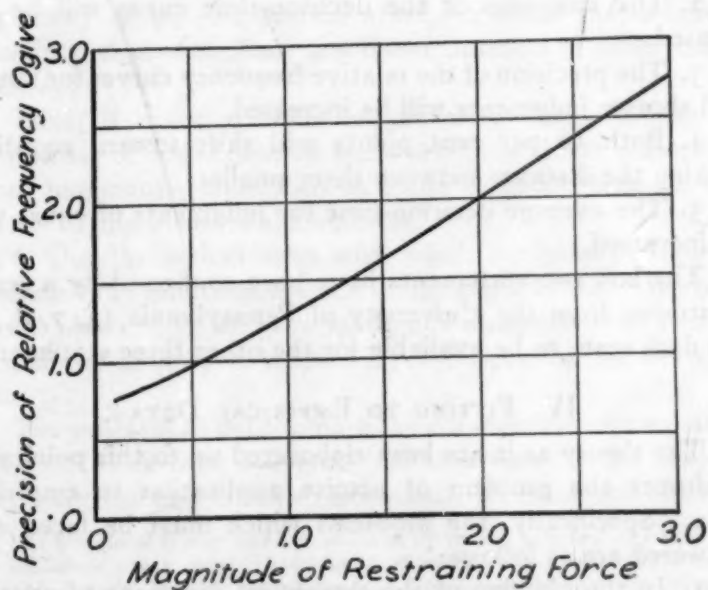h are obtained when the restraining forces are the same in both directions, and of a magnitude equal to the average of the unequal restraining forces.

Derivation 5a.—Under the conditions described in Derivation 5, the 50 per cent point is shifted towards the side of the greater restraining force by a distance along the abscissa equal to one-half the difference between the restraining forces.

Assuming that in a three category psychophysical experiment (judgment of equal allowed) instructions which create an attitude against giving judgments of equal increase the restraining force against "saying 'equal,'" the following statements can be made:

Under conditions where an attitude against giving a judgment of equal is created:

1. The maximal decision-time on each side of the equal category will be increased.

2. The steepness of the decision-time curve will be increased.

3. The precision of the relative frequency curves for longer and shorter judgments will be increased.

4. Both 50 per cent points will shift toward equality, making the distance between them smaller.

5. The average decision-time for judgments of equal will be increased.

The last two statements have been confirmed by a series of studies from the University of Pennsylvania (3, 7, 8, 9). No data seem to be available for the other three statements.

IV. Fitting to Empirical Data

The theory as it has been elaborated up to this point still confronts the problem of precise application to empirical data. Specifically, the problems which must be faced and answered are as follows:

1. In the plotting of the theoretical functions of relative

frequency and decision-time we have used a scale of arbitrary units in stating the magnitude of the mean of the z distribution. Theoretically these units are linearly related to differences in the potency of situations in which longer or shorter is correct. In order to convert our theoretical abscissa into an empirical one, some quantitative, operational definition of potency must be made. To meet this need the following operational definition is put forward: The confidence with which the subject makes his judgment may be taken as a measure of the difference in potency between the two overlapping situations. The confidence reflects the subjective probability that the judgment is correct. For example, if the subject gives a judgment of longer, but has absolutely no confidence in the correctness of his judgment, then the difference in potency is zero: "longer being correct," has a potency of .5 and "shorter being correct" has a potency of .5.[5] If the subject gives a judgment of longer and is absolutely confident that his judgment is correct, then the difference in potency is equal to 1; the situation in which longer is correct has a potency of 1; and the situation in which shorter is correct has a potency of 0. We further state, to make the definition complete, that confidence is a linear function of difference in potency.

Thus, if in the process of gathering data we also get judgments of confidence on some arbitrary linear scale, then these judgments, multiplied by some constant, may be converted to our theoretical z abscissa.

2. Our theoretical units with which we have plotted our decision-time functions are not in units of seconds. We have stated above that we have made our units relate to seconds in a linear manner as defined by the equation for decision-time.

The problem of determining the constants in this equation remains. The base constant (reaction time of the subject) may vary from situation to situation. The method of deter-

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[5] At first glance it may seem impossible to obtain any judgment at a time when the confidence equals zero. Theoretically, of course, this is correct. It is here assumed, however, that the subject can give a confidence rating which will reflect the average potency for the duration of the decision.

mining this base constant is defined as follows. That value of decision time which is obtained when the potency of one response's being correct is 1, and the other, 0, is taken as the base constant for our conversion into seconds. The rationale for the above procedure consists in the fact that when the difference in potency is 1, then there is no conflict of forces and we obtain the equivalent of an 'immediate' decision.[6]

3. The constant of conversion into units of seconds must of course be determined empirically by orthodox methods of curve fitting. The following restriction must, however, be observed in regard to the constant of conversion into seconds and the constant of conversion of the confidence scale into the z scale. Once such a constant is determined for a given subject under some condition, this constant must also hold for that subject under all other conditions. This restriction is meaningful for if these constants do not remain the same for the same individual under different conditions, then these constants would cease to have any theoretical meaning but would become merely the finding of a number which will make a curve fit. If our definition of psychological time is related to physical time units, then the same constant should be used in converting it to time units, at least for the same subject. And if our confidence scale really measures differences in potency, then the same difference in the confidence scale should refer to the same potency differences for the same individuals.[7]

4. The last problem which must be faced is the determination of the value of restraining force which is present for any set of empirical data. This determination is largely a

[6] In instances of extremely high restraining forces, this statement is not absolutely correct. In such cases even decisions at a potency difference of 1 will be slightly delayed beyond the reaction time. The amount of this delay, however, is very small and may either be ignored or taken into account by subtracting an empirically determined value from the actual decision-time.

[7] For the same subject the psychological unit of time might change drastically because of some change in the meaning of the situation for the subject. Also, since the constant of conversion of the confidence scale into the z scale depends upon the meaning which the subject gives to his confidence scale, it is easy to imagine a situation where the confidence scale changes markedly in meaning necessitating a change of the constant. We do not, however, consider such marked changes to occur very frequently.

matter of trial and error.   It is not, however, a value which
may be juggled around independently of the other values
involved.   To illustrate this procedure let us follow through
a hypothetical case of determining the restraining force value
for a given set of data.   There are two empirical functions
whose exact shape will be determined by the restraining force
magnitude, namely, decision-time and relative frequency
plotted against a scale of confidence.   It will be remembered
that all of the relative frequency curves are normal ogives
theoretically, but that the precision of the ogives decrease
with a decrease in the magnitude of the restraining force.
And we know exactly what the value of the standard deviation
in z scale units is for every value of restraining force (Fig. 5).
Therefore, if we determine the standard deviation of our
empirical relative frequency curve, the choice of any value
for the restraining force immediately fixes the constant of
conversion of the confidence scale into the z scale along the
abscissæ.   Then, with this conversion already determined,
the decision-time curve must also fit.   Figure 3 shows that
the steepness of the decision-time curve increases with an
increase in the magnitude of the restraining force.   Therefore,
in the fitting of the data, a choice of restraining force value
which is too low will yield a theoretical decision-time curve
which is less steep than the empirical one.   The choice of a
too high restraining force value will yield a theoretical
decision-time curve which is steeper than the empirical one.
The correct value of the restraining force, then, lies somewhere
in between and must be found by trial and error.   Then the
correct constant of conversion of theoretical decision-time
units into units of seconds may be determined.   Now, for all
other conditions of the same subject there are no more con-
stants to be determined.   Everything has already been set:
the time conversion constant, the conversion constant along
the abscissa, and the value of the restraining force for any
other condition (for now the standard deviation of the em-
pirical relative frequency curve defines the standard deviation
of the theoretical ogive which in turn defines the magnitude
of restraining force).

Thus, the empirical tests of the derivations made from the theory become very rigid, quantitative tests.

V. Review of Data Pertinent to the Theory

Some additional data, already existing in the literature, can be examined from the point of view of a tentative evaluation of the theory presented above.

Johnson (15), in a typical psychophysical experiment, dealt with the usual two categories of judgments. Measures of both relative frequency and decision time were obtained. In addition, ratings were obtained of the confidence with which the decisions had been made. These confidence ratings were made on a linear scale from zero to one hundred. Three sets of instructions were employed: The subjects made judgments under the usual set of instructions, under instructions emphasizing accuracy, and under instructions emphasizing speed.

The confidence ratings obtained in this way are identical with our operational definition of differences in potency. The accuracy and speed instructions are factors which we may assume will respectively increase and decrease the magnitude of the restraining force from what it was under the usual instructions. Thus, the experiment forms a convenient vehicle for a tentative test of our theory.

Unfortunately, however, there were only ten judgments for each stimulus value. The results are therefore somewhat unreliable relative to the precision of the theoretical predictions. Further, and more serious, is the fact that the data for relative frequency are presented only for the condition of usual instructions, and then not in a form convenient for our purposes. Without the relative frequency data, fewer checks on our theoretical derivations are available.

Figure 6 gives the theoretical curves drawn to the empirical data for decision time for one of Johnson's subjects under the three conditions described above. The conversion of confidence to the z scale along the abscissa is constant under all three conditions. The equation for this conversion is $C = 32z$. The constant for the conversion of the theoretical

decision-time curves into units of seconds is also the same for all three conditions. This constant is 11.3. The base constant is different for the three conditions as should be expected if the change in attitude affects reaction time. It is interesting to note, however, that the base constants are about the same for the accuracy and usual instructions.

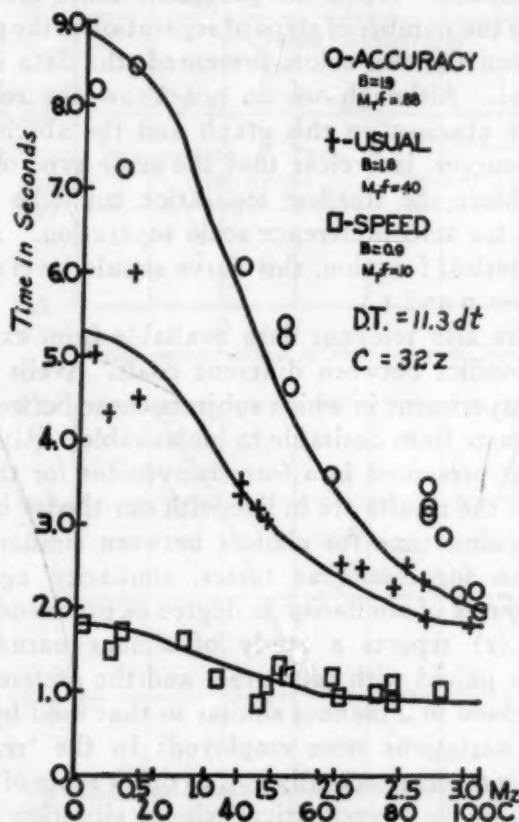Fig. 6. Theoretical decision-time curves fitted to data from one subject under three conditions of judgment. (Recalculated from Johnson, 15.)

While for the usual instructions the restraining force is .40, under conditions emphasizing accuracy it goes up to .88 and with instructions emphasizing speed it drops to .10. How well the theoretical curves fit the empirical data may be judged by the reader for himself.

There are also data showing that the relationships derived from the theory hold in areas outside of psychophysical judgments. Dashiell (6) in an experiment on affective judgments found results consistent with the theory. A number of colors were compared with each other and the rank order of preference established on the basis of the number of times each color was chosen. When the judgment times are arranged according to the number of steps of separation in the preference series between the two colors presented, the data in Fig. 7b are obtained. Although we do not know the relationship between the abscissa on this graph and the abscissa of our theoretical curves, it is clear that the same type of function obtains. (Since the smallest separation could be one there is no value for zero preference scale separation. According to the theoretical function, this curve should level out somewhat between o and 1.)

There are also relevant data available from experiments involving conflict between different goals. Wells (19) conducted an experiment in which subjects chose between liquids ranging in taste from desirable to undesirable. Although the data are not presented in a form convenient for the present comparison, the results are in line with our theory in that the average decision time for choices between similar tastes is greater than for dissimilar tastes, similarity again being defined in terms of similarity in degree of preference.

Barker (1) reports a study of similar nature. Seven liquids were paired with each other and the preference rankings determined in a manner similar to that used by Dashiell (6). Two variations were employed: In the 'real' choice situation the subjects actually had to drink some of the liquid they chose; in the 'hypothetical' choice situation they were required to choose the liquid they would prefer to drink if required to do so. It is clear that the restraining forces against making a decision are stronger in the 'real' choice situation than in the 'hypothetical' choice situation. The data for these two groups are reproduced in Fig. 7a. As in the case of the graph of Fig. 7b, the relation between this empirical abscissa and our theoretical abscissa is unknown.

Again, however, it is obvious that the curves obtained are of the same general type as the theoretical curves. In addition, in line with the theory, the curve of the 'hypothetical' choice

Fig. 7a (Top). Time elapsing during the resolution of conflict between alternatives separared by different "distances" in the preference series. (From Barker, 1.)
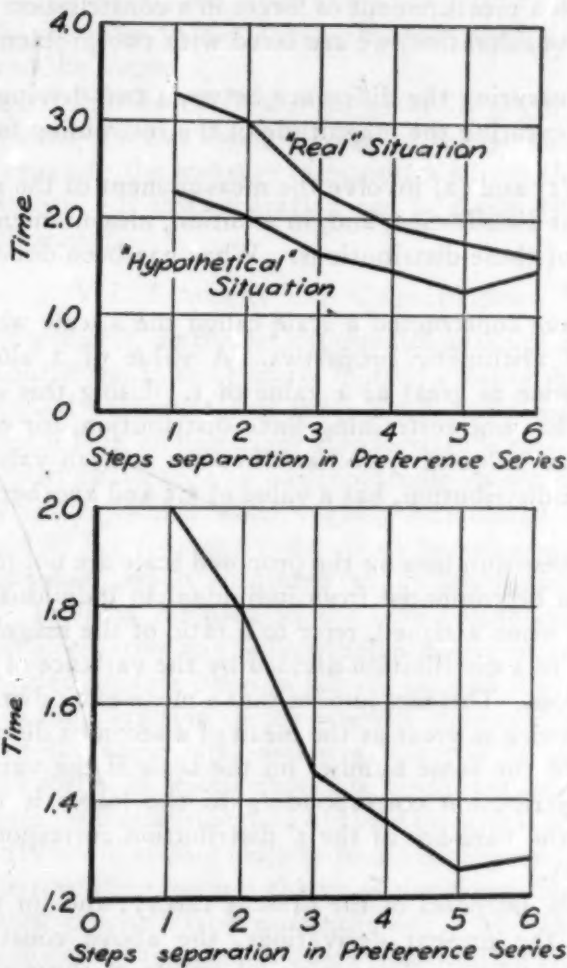
Fig. 7b (Bottom). Time elapsing for esthetic judgments between alternatives separated by different "distances" in the preference series. (From Dashiell, 6.)

situation shows a lower maximal decision time and less steep slope than the curve for the 'real' choice situation.

VI. RELEVANCE TO MEASUREMENT OF FORCES

How far the present type of analysis has gone toward measuring forces is still a matter for consideration. To accomplish a measurement of forces in a constellation such as is under consideration, we are faced with two problems:

(1) Measuring the difference between two driving forces.
(2) Measuring the magnitude of the restraining force.

Both (1) and (2) involve the measurement of the mean of the normal distribution, and, in addition, measurement of the variance of these distributions. What has been done in this direction?

We have constructed a scale called the z scale which has the usual arithmetic properties. A value of 2 along this scale is twice as great as a value of 1. Using this scale we can say that one restraining force distribution, for example, has a mean value of .9 and another has a mean value of .5; that one z distribution, has a value of 1.5 and another a value of .8.

But these numbers on the proposed scale are not measures which can be compared from individual to individual. The numbers, when assigned, refer to a ratio of the magnitude of the mean of a distribution divided by the variance of another distribution. This fact implies that a mean of a z distribution which is twice as great as the mean of a second z distribution is assigned the same number on the scale if the variance of the z' distribution corresponding to the latter is twice as great as the variance of the z' distribution corresponding to the former.

For the purposes of the present theory, and for the purposes of the present derivations, the above constitutes a correct procedure. The empirical effects of these two situations would be identical. From the point of view of measurement of magnitude of forces, however, this procedure is inadequate. For the same individual in two different situations it does constitute a real scaling of magnitudes of forces if we assume that the variance of the z' distribution (our unit

of measurement) stays the same for that individual in the two situations. For a comparison between different individuals between whom the variance of the z' distribution is not equal, however, we must know the relative size of the variances in order to say anything about the relative magnitudes of the forces.

The possibilities of further progress in the direction of measurement of forces seems to hinge on the question of being able to separate the measure of magnitude from the measure of variability. If this separation can be made, the problem is largely solved.

VII. Summary and Conclusions

The successful, mathematical quantification of a scientific theory constitutes to some degree in itself a test of that theory. If the logical prerequisite of inner consistency contained in such a test is met, an important step forward is made. In the development of science this has repeatedly shown itself to be true. Theories, which when vaguely formulated appear to be adequate, are frequently shown to be self contradictory and inadequate when subjected to an attempted quantification. Even as far back as the seventeenth century the Cartesian alternative to the Newtonian theory of gravitation was discarded because Newton himself showed that a quantification of the Cartesian theory was self contradictory.

Except for some rare instances, theories in psychology have been stated in relatively vague, nonquantitative terms. When more rigid quantification of these theories is attempted, many of them will undoubtedly be found inadequate.

In the preceding pages a theory of decision situations has been stated precisely and quantitatively. The theory has been developed to that point where quantitative derivations from the theory are capable of being subjected to direct, quantitative empirical test.

We may summarize as follows:

1. A theory of decision which combines a topological analysis with a vectorial analysis of a decision situation has been elaborated mathematically.

2. Through this mathematical elaboration a successful quantification of this theory was accomplished in a form amenable to empirical testing.

3. The type of theoretical analysis used has methodological significance. Irrespective of the school of thought, or of the theoretical constructs involved, this type of approach can be used to measure the dynamic factors in a psychological situation. From measures of frequency and decision time we may infer the magnitudes of the forces operating. This type of analysis is applicable to a wide variety of psychological problems.

Experiments specifically oriented toward testing the quantitative predictions from the theory presented in this paper are reported elsewhere by Festinger (**10**, **11**).

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ON THE OBJECTIVITY OF ÆSTHETIC VALUES

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If one were to list the more aggravating sources of difficulty in modern philosophical inquiry he would include without hesitation the oft-made but never clearly understood distinction between the *subjective* and the *objective*. This distinction, whenever it has been made, has brought in its wake a dualistic conception of the nature of things, which latter, in turn, has in more instances than not led to the frustration of philosophical inquiry. The subjective-objective distinction, when used with the method of ontological postulation, has created the setting for that still perplexing set of difficulties which we casually refer to as the 'mind-body problem.' Further, when an examination is made of those instances in which this distinction is employed one finds that there is no common agreement among those who make the distinction as to what constitutes the subjective and the objective, respectively. Sometimes the objective is conceived as whatever exists independent of the experiencing knower; the subjective being defined as that which depends for its existence upon the knower. But this is never satisfactory when stated so simply, for while it seems quite illuminating to have it pointed out that there are elements in our knowledge to which we acquiesce, and that there are other elements which spring from the participation of the knower in cognitive processes, the rub is, how much of our knowing is acquiescence, and how much of it is participation? Which elements of our knowledge are given and which taken? Frequently, the definition of the objective as that which has an independent source of being and which is given to us to acquiesce to, is confused with that which exists external to our feeling and thinking, external to the physical periphery of our being.

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This identification of the objective with the external does not solve the problems raised by the initial distinction between the subjective and objective—when this distinction is laid down according to the method of ontological postulation—any more than does the identification of the objective with the given.

But these remarks apply primarily to epistemologico-metaphysical discussions of the subjective-objective distinction, and in the past this type of discussion has held no monopoly on the bifurcation of reality of which we have been speaking. In the field of value theory one often finds that the term 'objective' is used in discussions of standards of value in two different senses: On some occasions 'objective' refers to the independent existence of standards and on other occasions it refers to the absolute nature of standards. The impetus towards identification of objectivity with the independent existence of values derives from the desire to know what is intrinsically and universally valuable, a desire which is deeply rooted in individual human efforts to be accepted by others, to achieve prestige, and in other social motives. The identification of objectivity with absoluteness in standards of value has several motivations, not least of which is the desire for that finality in standards which gives security to individual value judgments based on absolute standards. The desire for the security of absolute standards of value has more often than not overridden all the empirical evidence we have on this matter and which conclusively points to the non-existence of such standards; in such instances ontological postulation supersedes empirical verification.

Now what has been so briefly presented is only a sampling of the difficulties that attend the ontological postulation of a subjective realm of being and an objective realm of being. Further investigation would reveal that while the subjective-objective distinction is often ontologically postulated, there is no common or consistent set of meanings that attaches to each of these two terms, with the result that more confusion than clarification has been fostered in our thinking by the making of the distinction. It would seem in order then, that

an examination be made of what is meant by objectivity and subjectivity in philosophical inquiry. Here we shall have to limit ourselves to a brief examination of what is meant by objectivity in values in general and in æsthetic values in particular. We shall discuss first the nature of the value-situation and second, the sources of objectivity in valuational processes.

A value-situation involves three factors: an interested organism, the object of its valuation, and valuational processes which involve and are determined by the former two. An adequate account of the value-situation and the values which emerge from it will necessarily include accounts of all three of these factors. The chief underlying fallacy of 'subjective' theories of value, for example, lies in their attempted account of values solely in terms of the interested organism which is initially misconceived as some kind of self-contained entity with static properties. The basic inadequacy of most so-called 'objective' theories of value can usually be traced to an over-emphasis upon analysis of the nature of the object of valuation, while fixed conceptions of the interested organism and valuational processes are assumed. Here we shall not be able to go into detailed accounts of all three factors in the value-situation but must be content, rather, with a few fundamental characterizations. This should not blind us to the necessity for a full understanding of all three factors in the value-situation before we can achieve an adequate account of values.

First, with regard to the interested organism, it should be noted that while interest (liking, seeking, desiring, and their opposites, disliking and avoiding) is a necessary condition of the emergence of values, it is not a sufficient condition. Valuational processes are preferential processes involving the operation of intelligence. In this connection there are some value theories that are unacceptable for the reason that they begin by identifying valuation with interest, thus over-simplifying the value-situation; and in their efforts to preserve a continuity between human and less complex forms of behavior these theories usually involve consideration of such

absurdities as electrons changing their orbital movements out of sheer boredom, or magnets valuing iron filings. Second, the referential nature of interest is of basic importance. Interest never exists *in vacuo* but is always desire for or interest in something. This referential nature of interest is the foundation for our definition of what is meant here by 'objective.' The referential nature of interest suggests that 'subjective' and 'objective' are correlative and polar terms indicating perspectives from which the valuational process may be viewed. On this account 'subjective' and 'objective' are not characterizations of entities or substances, but are rather descriptive terms applicable to distinguishable but inseparable aspects of a field of human activity—in this instance, the process of valuation.

Another fundamental characterization of the value-situation is that it is a type of meaning-situation; valuations occur only in meaningful situations. This characterization has at least two important sets of implications for an adequate understanding of values and how they emerge out of human behavior. First, there are at least three distinguishable but inseparable types of meaning that a significant situation has for the human organism endeavoring to interpret that situation. Interpretation of a meaningful situation by an organism may be primarily oriented towards a discovery of the intelligibility of that situation, or towards its existential implications, or towards its affective significance. The process of interpretation, which underlies judgments of fact and judgments of logical and other values, always includes attention to all three types of meaning, but from one valuational process to another, attention is directed primarily along one or two of the three dimensions of meaning. In science, attention is directed primarily along the intellectual and existential dimensions of meaning and the affective dimension is de-emphasized. In judgments of æsthetic value, attention is directed along the affective dimension with the existential and intellectual dimensions receiving varying degrees of lesser emphasis.

This analysis brings us to the second set of implications

that follow from the fact that value-situations are a type of meaning-situation. We refer here to an early distinction made by Dewey between referential and immanent meanings. Meaning as referential is relational. It occurs wherever and whenever one thing is functioning or serving as the sign of another. In such referential instances we speak of the consequences of an event as that which the event signifies. Now when the possible consequences of an event become identified with that event as its properties, the referential meanings of the event have 'collapsed' into immanent meanings, and the event may be said to have 'acquired' meaning. This distinction between referential and immanent meanings affords the basis of an interesting approach towards the understanding of ideals or standards of value. Ideals or standards of value are values which, over a period of time, either in the life of an individual or a society (for it is possible to speak of social ideals), have become saturated with immanent meanings. A standard of value or ideal, the meanings of which have become increasingly immanent and decreasingly referential, comes to be regarded as absolute, perfect, and objective (in the sense of being regarded as independently existent).

One more characteristic of meanings is worthy of our attention. All meanings are potentially intersubjective, *i.e.*, while there are personal or private aspects of the experiencing or being aware of meanings, meanings are not exclusively private on the level of cognition and valuation. With this note on the potential intersubjectivity of all meanings we now turn to some of the sources of objectivity in the processes of valuation.

One source of objectivity in all value judgments, and thus a source of objectivity of the values that are constitutive of and emergent from these judgments, lies in the social factors influencing and determining valuational processes. In brief, we regard those values as objective which are the products of judgments concurred in by others whom we regard as competent to make such judgments. Almost immediately there comes to mind the social cooperation employed by scientists in their mutual determination of such methodological and

logical values as factuality and truth. What an individual scientist comes to value methodologically as constituting the facts and the true hypotheses of his field of investigation is determined in large part by what is concurred in and verified by other scientific observers and investigators whose competence he regards as established. The æsthete who judges certain art objects as æsthetically valuable—or more properly speaking, who judges certain art objects as capable of eliciting the experience of genuine æsthetic values—likewise looks to the concurring judgments of competent æsthetically sensitive persons on those same art objects. Thus one source of objectivity in all values, including æsthetic values, is the concurrence of value judgments among persons who mutually regard each other as competent to make such value judgments.

This is not to say that good art, for example, is solely a matter of convention, for the factors of sensitivity and competence among æsthetes, as in the case of competence among scientists, are determinable—if not as yet in a completely quantitative manner—by means that have been developed through the years. In fact, these effective means of establishing æsthetic sensitivity and competence are assumed whenever we expose the idosyncrasies of art-school and art theory cliques and the æsthetic abortiveness of a current academicism.

Another source of the objectivity with which we like to clothe our value judgments is to be found in our tendency to generalize those judgments. It is not satisfactory enough for us to make value judgments and to cling to them without caring whether others concur or not. What an individual judges as of value he tends to impute to the judgment of others. From his own judgment of what is valuable he goes on to regard his values as values for all; what is good or beautiful or true for him must be good or beautiful or true for all others. If the individual does not find ready concurrence with his value judgments, he will try to communicate to others the meanings upon which he has built those judgments. This he does with the hope that others, becoming aware of those meanings, will concur with him in the value judgments

that he has based on those meanings; he seeks concurrence in value judgments on the basis of concurrence in interpretation. If this fails, he may question the validity of his own value judgments and cast them aside for judgments new to him but on which he finds concurrence. Or, persisting in his belief in the basic validity of his value judgments, he may become either a recluse or a reformer—or both alternately. In any event, the tendency to seek objectivity for one's value judgments feeds upon the tendency to generalize one's value judgments. No man will long retain conviction in his value judgments if he sincerely believes that his judgments are purely private and are not applicable to the lives of others. A genius, however, who persistently maintains belief in the validity of his value judgments in the face of opposition, does so rightly—as history so often demonstrates—because he has greater knowledge of what is valuable for both himself and the rest of mankind; the apparent egotism of true genius is often belatedly recognized for the wisdom that it is. The chief difference between the generalizing of us lesser lights and that of the genius is that ours is usually no more than egotism while the generalizing of the genius rests on a wisdom that gives validity to the process.

Mention was made earlier of the tendency to identify the objective with that which is external to the organism. This has particular significance in the making of æsthetic value judgments. Here also, the referential nature of interest—mentioned earlier—is pertinent. If what an individual regards as valuable exists external to himself, he has a tendency to regard the object or event valued as intrinsically valuable, i.e., as having values regardless of his personal judgment. In such instances what elicits a valued experience is so obviously 'out there' that there is a tendency on the part of the observer to project the elicited valued experiences into the object itself and thus to regard those values as properties of the object. Our language itself betrays this tendency. We say that Beethoven's symphonies are great music, that Giorgione's 'Pastoral Concert' is a fine painting, and on almost every occasion, when speaking of valued æsthetic

experiences we make constant reference to the objects and events that have elicited those experiences rather than to the experiences themselves.   Again, there is a kind of 'confirmation' of this objectification of æsthetic values in the fact that a particular eliciting object or event may be experienced repeatedly with æsthetic effects on each occasion.   The æsthetic values involved being precious, the individual wants some permanency about them and thus tends to identify the valued experiences with the external eliciting object or event which persists 'out there' in some form or another throughout the subsequent changes in his conscious experience.   This confirmation of the externality type of objectivity which the individual attributes to æsthetic values is further strengthened when he finds that he can discuss the same æsthetic values with others as they are elicited by an identical object or event.

Another source of objectivity is to be found in the process of identification which occurs in æsthetic experiences.   Various æstheticians have pointed out the factor of detachment, of psychic distance, of disinterested interestedness, which plays such an important role in all æsthetic experiences.   In the presence of an object or event which elicits an æsthetically valuable experience the organism experiences that object or event for what it is and without primary reference to its historical, intellectual, or practical connotations.   In this attitude the experiences of the organism become fused with the eliciting object or event; an intensity of detached attention is achieved in which the æsthetic experience and that which elicits it become identified.   After the experience, the eliciting object or event is still there (actually as in the visual arts or potentially as in music or drama) while the experience itself has gone.   There is a tendency to find during the aftermath of the æsthetic experience something relatively permanent to which may be attached the significant experience just gone through.   The eliciting object or event, as all that substantially remains, is seized upon and regarded as containing the æsthetic values experienced—the æsthetic values experienced are regarded as properties of the eliciting object or event. And this objectification the individual does not find it difficult

to do when we consider the fact that while the æsthetic experience was going on there existed no clear distinction for him between the valued aspects of that experience and the object or event which elicited the experience.

Still another source of objectivity is to be found in the completeness and unity of the æsthetic experience. The æsthetic experience, more than any other type of experience, is unified, integrated, complete. Particularly is this true of the sensory form which is experienced of those objects and events which elicit æsthetic values. The sensed completeness of the sensory form becomes the occasion for the individual to attribute some kind of independent existence—as 'essences,' properties of externally existing objects or events, and the like—to the concomitantly experienced æsthetic values which are fused with the sensory form in the wholeness of the æsthetic experience. What is complete, unified, integrated is regarded as somehow self-sufficient and independently existing. Thus the unity in variety, the completeness, the integrated nature of the sensory form in the æsthetic experience leads to the identification of the 'whole' qualities of the sensory form with the æsthetic values experienced. And the tendency to regard the sensory form as independently existing leads to an objectification of the æsthetic values involved in the experience.

There are probably sources of the objectivity of æsthetic values other than those which have been mentioned here, but we have outlined enough of the more significant sources to indicate that the problem of the objectivity of æsthetic values—and of all values, for that matter—must be approached from a point of view other than that of the method of ontological postulation. Now much of what has been said here will be glibly dismissed by some persons as an example of 'psychologism.' To these we may reply that whatever label they wish to attach to the suggestions given here, these suggestions, we hope, offer a new point of view towards the subjective-objective distinction; a point of view dedicated to clearing away much of the befuddlement that has been created by past 'ontologism' in value theory and æsthetics.

What is given here does not presuppose anything which is not scientifically known nor does it involve assumptions or facts inconsistent with present scientific knowledge. The referential conception of the subjective-objective distinction offered here recommends itself to those who are concerned with developing scientific æsthetic theory and with freeing æsthetics from the misleading and outmoded substantive conception of the subjective-objective distinction which is a product of the method of ontological postulation. The ontological postulation of the objectivity of æsthetic values—an objectivity which is defined in various ways, depending upon the ontology involved—has in the past led to more confusion in æsthetic theory than it has to the progressive development of that field of inquiry. What has been presented here suggests the possibilities of a new approach to a better understanding of the existential status of values. An approach such as this, involving referential definition of the factors involved in valuational processes, is, we believe, a superior approach to the problem of the existential status of values over the misleading and arbitrary methods of ontological postulation.

JAMES BURT MINER
1873–1943

Dr. Miner's work in psychology, which spread over 40 years, centered around the applications of psychology in the fields of delinquency, guidance, and selling. He got into psychology in a roundabout fashion. Immediately after graduating from the University of Minnesota in 1897, he became a newspaper reporter by day and a law student by night. He returned two years later to Minnesota to obtain his M.S. degree. In 1901 he went to Columbia University as a university fellow under Professor James McKeen Cattell, and in 1903 obtained his degree. His interest in individual differences and the application of psychology is evident from his dissertation.[1] He wrote, "The intimate relation between rhythm and work opens numerous inviting avenues of experiments," and, "The slow person, under rhythmic stimulation, tends to improve in efficiency while the quick person tends to lose very decidedly."

He went from Columbia to the University of Illinois, substituting in part for Professor S. S. Colvin who was on sabbatical leave. He was, I believe, the first full-time instructor in psychology at Illinois. While there he published an article on the changing attitude of American universities toward psychology. This article was typical of Dr. Miner. Psychology as a separate discipline was just beginning in American universities, and he enjoyed being in on the ground floor. Throughout his professional career he would begin many new kinds of psychological activities; and once they were begun, he would turn them over to others and begin something else.

From Illinois he went to the University of Iowa, where he worked with a young woman who had been blind until

[1] Motor, visual and applied rhythms. *Psychol. Rev. Monogr. Suppl.*, 1903, **5**, No. 21.

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maturity when an operation restored her sight. She disappeared from the hospital before he made as thorough a study as he had planned. The results of this investigation were published, and the ensuing controversy with some of the older men had a profound effect upon him. He became cognizant of the fact that some men whose theories were invalidated by facts were not adverse to impugning the honesty of others.

He next went to Minnesota, where he remained for nine years. Here his interests in applied psychology definitely asserted themselves. He founded a psychological clinic—the first west of Chicago. He became consulting psychologist for the Minneapolis Juvenile Court. From 1910 to 1920 he was a collaborator for the *Journal of Educational Psychology*.

From Minnesota he went to the Carnegie Institute of Technology. There he acted as executive secretary and the first director in organizing the Research Bureau for Retail Training. He was also a consultant to the Bureau of Salesmanship Research. At the University of Kentucky (1921–1943) he organized both a psychological clinic and a Personnel Research Bureau. When he came to Kentucky he was the whole psychology department; when he died the departmental staff consisted of 12 members. He organized a state-wide testing program for high school students, and at the time of his death was working strenuously on two other state-wide programs: mental hygiene and guidance.

In the field of psychology Dr. Miner is noted chiefly for his book, *Deficiency and delinquency*,[2] and his interests inventory in which he used the paired-contrast technique,[3] although his investigation of vision acquired in adult life [4] and his annual reviews of correlation in the *Psychological Bulletin* for a period of over ten years deserve mention.

A year before Dr. Miner died, he took the Kuder Prefer-

[2] *Deficiency and delinquency, an interpretation of mental testing*, Baltimore: Warwick & York, 1918, pp. xiv–355.
[3] An aid to the analysis of vocational interests. *J. educ. Res.*, 1922, 8, 311–323.
[4] A case of vision acquired in adult life. *Psychol. Rev. Monogr. Suppl.*, 1905, 6, No. 6, 103–118.

ence Record and scored above the 90th percentile in social service interests and just below it in computational interests. His intense desire to help people, to aid them in solving their problems, to change society to suit the needs of all individuals, and to achieve these ends in a thoroughly objective fashion dominated him for some fifty years.

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AMERICAN PSYCHOLOGICAL PERIODICALS

American Journal of Psychology—Ithaca, N. Y.; Cornell University. Subscription $6.50. 624 pages annually. Edited by K. M. Dallenbach, Madison Bentley, and E. G. Boring. Quarterly. General and experimental psychology. Founded 1887.

Journal of Genetic Psychology—Provincetown, Mass.; The Journal Press. Subscription $14.00 per annum (2 volumes). 1000 pages annually. Edited by Carl Murchison. Quarterly. Child behavior, animal behavior, and comparative psychology. Founded 1891.

Psychological Review—Northwestern University, Evanston, Illinois; American Psychological Association, Inc. Subscription $5.50. 540 pages annually. Edited by Herbert S. Langfeld. Bi-monthly. General psychology. Founded 1894.

Psychological Monographs—Northwestern University, Evanston, Illinois; American Psychological Association, Inc. Subscription $6.00 per volume. 500 pages. Edited by John F. Dashiell. Without fixed dates, each issue one or more researches. Founded 1895.

Psychological Bulletin—Northwestern University, Evanston, Illinois; American Psychological Association, Inc. Subscription $7.00. 665 pages annually. Edited by John E. Anderson. Monthly (10 numbers). Psychological literature. Founded 1904.

Archives of Psychology—New York, N. Y.; Columbia University. Subscription $6.00 per volume. 500 pages. Edited by R. S. Woodworth. Without fixed dates, each number a single experimental study. Founded 1906.

Journal of Abnormal and Social Psychology—Northwestern University, Evanston, Illinois; American Psychological Association, Inc. Subscription $5.00. 560 pages annually. Edited by Gordon W. Allport. Quarterly. Founded 1906.

Journal of Educational Psychology—Baltimore, Md.; Warwick & York. Subscription $6.00. 720 pages annually. Edited by J. W. Dunlap, P. M. Symonds, and H. E. Jones. Monthly except June to August. Founded 1910.

Psychoanalytic Review—New York, N. Y.; 64 West 56th St. Subscription $6.00. 500 pages annually. Edited by Smith Ely Jelliffe. Quarterly. Founded 1913.

Journal of Experimental Psychology—Northwestern University, Evanston, Illinois; American Psychological Association, Inc. Subscription $14.00 per annum (2 volumes). 1040 pages annually. Edited by Samuel W. Fernberger. Monthly. Founded 1916.

Journal of Applied Psychology—Northwestern University, Evanston, Illinois; American Psychological Association, Inc. Subscription $6.00. 480 pages annually. Edited by Donald G. Paterson. Bi-monthly. Founded 1917.

Journal of Comparative Psychology—Baltimore, Md.; Williams & Wilkins Co. Subscription $14.00 per annum (2 volumes). 1000 pages annually. Edited by Roy M. Dorcus. Bi-monthly. Founded 1921.

Comparative Psychology Monographs—Baltimore, Md.; Williams & Wilkins Co. Subscription $6.00 per volume. 400 pages. Edited by Roy M. Dorcus. Without fixed dates, each number a single research. Founded 1922.

Genetic Psychology Monographs—Provincetown, Mass.; The Journal Press. Subscription $7.00. 500 pages annually. Edited by Carl Murchison. Bi-monthly. Each number one complete research. Child behavior, animal behavior, and comparative psychology. Founded 1925.

Psychological Abstracts—Northwestern University, Evanston, Illinois; American Psychological Association, Inc. Subscription $7.00. 700 pages annually. Edited by Walter S. Hunter and H. L. Ansbacher. Monthly. Abstracts of psychological literature. Founded 1927.

Journal of General Psychology—Provincetown, Mass.; The Journal Press. Subscription $14.00 per annum (2 volumes). 1000 pages annually. Edited by Carl Murchison. Quarterly. Experimental, theoretical, clinical, and historical psychology. Founded 1927.

Journal of Social Psychology—Provincetown, Mass.; The Journal Press. Subscription $7.00. 500 pages annually. Edited by John Dewey and Carl Murchison. Quarterly. Political, racial, and differential psychology. Founded 1929.

Psychoanalytic Quarterly—Albany, N. Y.; 372-374 Broadway. Subscription $6.00. 560 pages annually. Edited by Bertram D. Lewin and others. Quarterly. Founded 1932.

Character and Personality—Durham, N. C.; Duke University Press. Subscription $2.00. 360 pages annually. Edited by Karl Zener. Quarterly. Founded 1932.

Journal of Psychology—Provincetown, Mass.; The Journal Press. Subscription $14.00 per annum (2 volumes). 800-1200 pages annually. Edited by Carl Murchison. Quarterly. Founded 1936.

Psychometrika—University of Chicago, Chicago, Ill.; Psychometric Society. Subscription $10.00. 320 pages annually. Edited by L. L. Thurstone and others. Quarterly. Quantitative methods in psychology. Founded 1936.

Psychological Record—Bloomington, Ind.; Principia Press. Subscription $4.00. 500 pages annually. Edited by J. R. Kantor and C. M. Louttit. Without fixed dates, each number a single research. General psychology. Founded 1937.

Journal of Consulting Psychology—Lancaster, Penn.; Science Printing Co. Subscription $3.00. 240 pages annually. Edited by J. P. Symonds. Bi-monthly. Founded 1937.